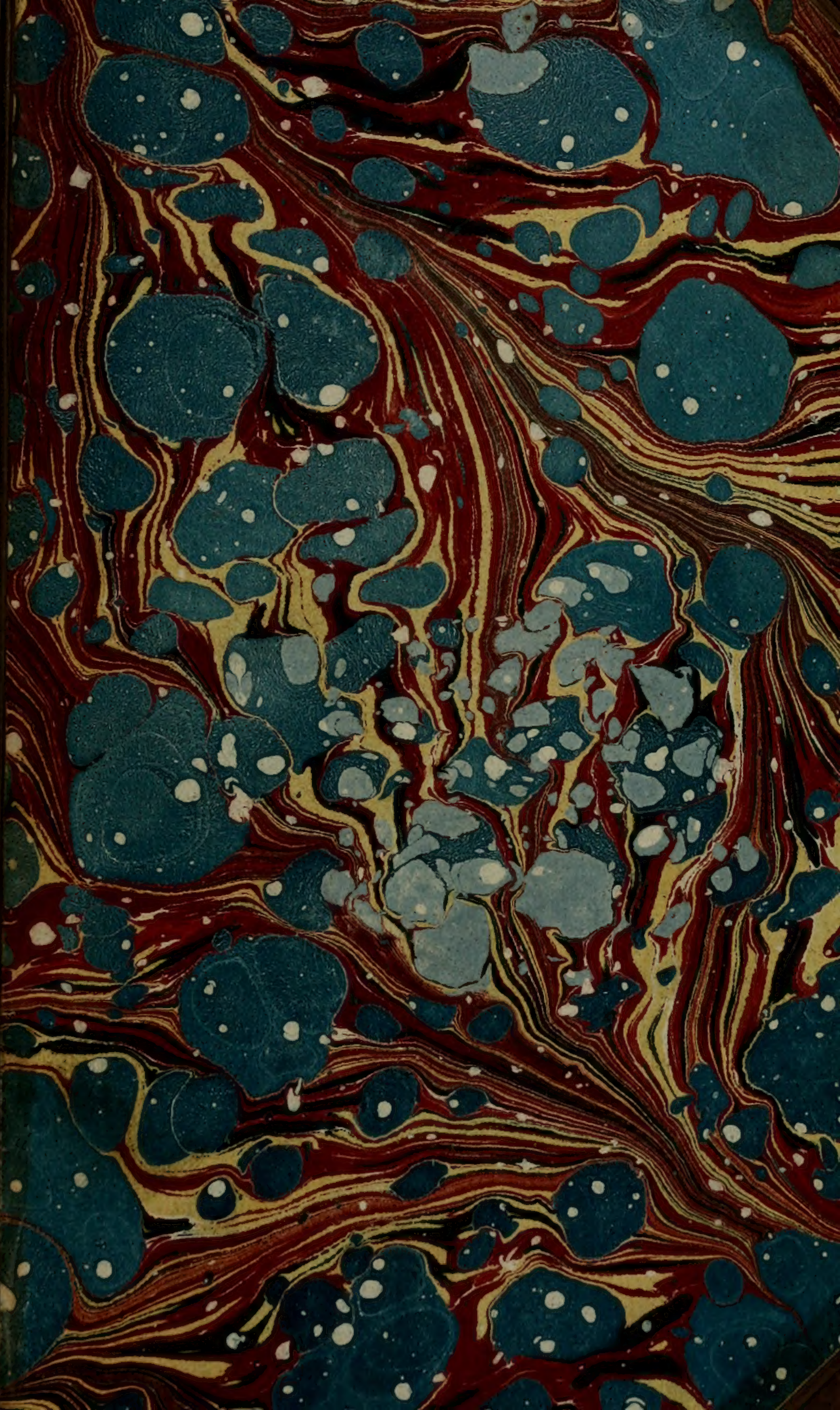

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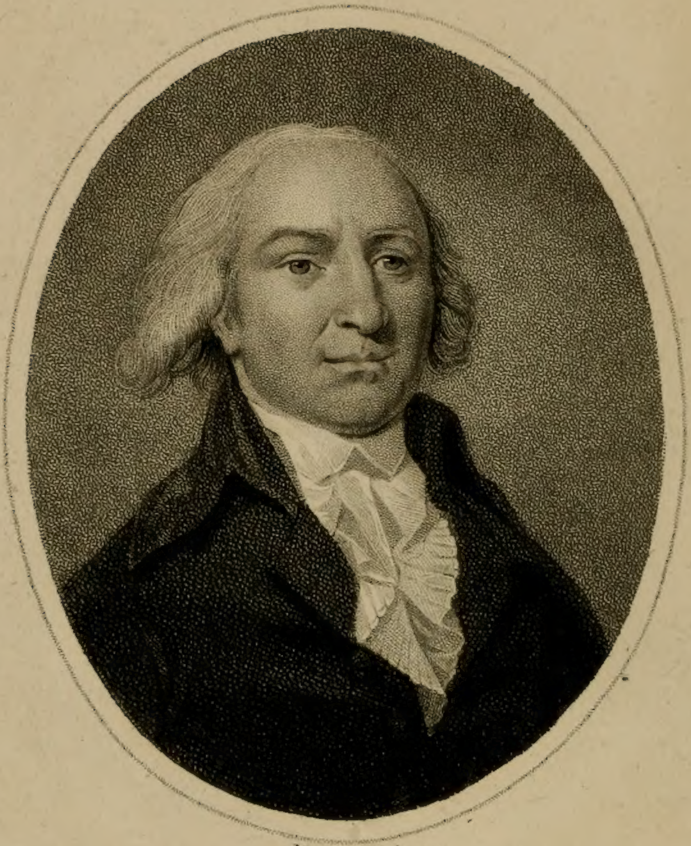

Mackenzie Sculp.

A. F. Fourcroy.

Publish'd by A. Tilloch Carey Street Jan^{y}. 31. 1802.

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

MEMBER OF THE LONDON PHILOSOPHICAL SOCIETY, ETC. ETC.

"Nec aranearum ſane textus ideo melior, quia ex ſe fila gignunt. Nec noſter vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XI.

LONDON:

PRINTED FOR ALEXANDER TILLOCH;
And Sold by Meſſrs. RICHARDSON, Cornhill; CADELL and DAVIES,
Strand; DEBRETT, Piccadilly; MURRAY and HIGHLEY, No. 32,
Fleet-ſtreet; SYMONDS, Pater-noſter Row; BELL, No. 148,
Oxford-ſtreet; VERNOR and HOOD, Poultry; HARDING,
No. 36, St. James's-ſtreet; BELL and BRADFUTE,
Edinburgh; BRASH and REID, Glaſgow;
and W. GILBERT, Dublin.

[*Wilks and Taylor, Printers, Chancery-Lane.*]

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THE
PHILOSOPHICAL MAGAZINE.

I. *A Diſſertation on the Hiſtory of Sugar. By Profeſſor* BECKMAN[*].

THOUGH Saumaiſe, Voſſius, Deuſing[†], and other learned men have written on the hiſtory of ſugar, yet no one who has read and examined the opinions, diſputes, and arguments of theſe authors will deny that it is ſtill very defective and obſcure. For this reaſon, and as no article of commerce, whether we conſider the immenſe quantity of it uſed in different parts of the world, the great number of perſons employed in the cultivation of it, or in preparing and ſelling it, or the large ſums expended annually in the purchaſe of it, ſeems more worthy of having the hiſtory of it cleared up, I reſolved to try whether, after the attempts of ſo many writers, I could produce any thing new or certain on the ſubject; eſpecially as many new obſervations, which may have eſcaped theſe authors, can now be drawn from books of travels, and works on natural hiſtory. It may alſo happen, that what the antients have ſaid on the ſubject can be rendered clearer and more intelligible by a new reviſion; for the hiſtory of ſugar has been involved, by learned men, in ſuch a confuſion of words and things foreign to it, that it coſts no little time and labour to ſeparate truth from probability.

As our ſugar is the eſſential ſalt of that plant called by botaniſts *ſaccharum officinarum*, or *arundo ſaccharifera*, the queſtion whether this plant was indigenous in that part of the world known to the antients, ought to be diſcuſſed before we inquire whether the antient Greeks and Romans were acquainted with ſugar; for, if it could be proved that the ſugar cane was tranſplanted in modern times from the new to the old continent, there would be no occaſion for diſpute. But this point, as might be eaſily ſhown, has been neglected by moſt of thoſe who have attempted to write on the hiſtory of plants; and ſuch an error has been lately committed by a man of learning, who entered into a diſcuſſion reſpecting the Greek and Latin names of the potatoe (*tuberoſum ſolanum*), which, without all doubt, was tranſplanted into Europe from South America. In regard to the ſugar plant, it is indeed certain that it grows ſpontaneouſly in many parts of Aſia and Africa; for it can be eſtabliſhed, by incontrovertible proofs, that ſugar was made in Europe before it was known in America[*]. It is poſſible that the Greeks and the Romans may have been acquainted with a plant abounding with ſweet and pleaſant juice which grew ſpontaneouſly in various places; and as the word *ſaccharum*, which, without doubt, ſignifies ſome ſweet juice, occurs frequently in the writings of the antients, it is worth while to inquire whether the *ſaccharum* of the antients was the ſame as that which we uſe at preſent.

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[*] From the *Tranſactions of the Royal Society of Göttingen*, vol. v.

[†] Salmaſii Exercitationes Plinianæ, p. 716. 916.; and Exercitationes de Homonymis, p. 109.

Iſ. Voſſii Obſervationes ad Pompon. Melam: *Hagæ Comit.* 1658. 4to. p. 268.

Antonii Deuſingii Faſciculus Diſſertationum: *Groningæ* 1660. 12mo. p. 479. Theophraſti Hiſt. Plantarum illuſtrata à Joh. Bodeo a Stapel. *Amſtelodami* 1664, p. 109.

What the antients called *ſaccharum*, *ſakcharon*, *ſakchar*, or *ſakchari*, was a white ſubſtance, ſimilar in conſiſtence to ſalt, and which, like ſalt, broke under the teeth. It had a ſweet taſte, but leſs ſo than common honey. It was found in Arabia Felix, but the beſt was found in India; and it was brought alſo from the remoteſt parts of Ariace and the country of the Borygazi. It was not made by art, but was produced by a reed, in the form of gum, or adhering to the canes in natural concretions. The largeſt of theſe with which the antients were acquainted were in ſize equal to about that of a walnut. In the time of Pliny, Dioſcorides, and Galen, it was employed only in medicine[†].

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[*] *Voyages de Monſ. Chardin*, à Rouen 1723, 8vo. vol. iv. p. 34:—Quant au ſucre, je crois qu'il y en a eu de tout tems aux Indes. Je ſai bien que cela eſt fort conteſté,—mais je tiens le contraire, fondé ſur ce que le ſucre croît partout dans les Indes aboudamment, aiſément, excellemment, et non pas comme les fruits que l'on tire des pais éloignez, qui ne viennent jamais ſi bien, lorſqu'ils ſont tranſplantez loin de leur ſol. Une autre raiſon, que j'ai encore plus forte, c'eſt que le ſucre ſe trouve nommé et ordonné en cent endroits des anciens écrits de medecine Indiens, Perſans, et Arabes.

[†] *Plinii Hiſt. Nat.* lib. xii. c. 8, edit. Hard. vol. i. p. 659: Saccharon et Arabia fert, ſed laudatius India; eſt autem mel in arundinibus collectum

This is all that the antients have left us in their writings reſpecting *ſaccharum.* It is indeed ſhort and defective, but let us try what certain information can be drawn from it. The name itſelf is of no importance in determining the queſtion, for antient names have often been applied to new things with which the antients were unacquainted. For this reaſon then, though the name be the ſame, there may be a great difference between our ſugar and that of the antients. Nor can any arguments be adduced from the origin of the word, ſince it is altogether unknown, and as there are ſome who believe, and not without reaſon, that this appellation was given to various vegetable juices found naturally in a concrete ſtate on the plants which produced them *.

The plant which produces our ſugar, as well as that which produced the *ſaccharum* of the antients, may, in ſome meaſure, be referred to that claſs of plants called in general reeds (*arundines*), and grows in the ſame parts of the Eaſt Indies and Arabia from which the antients procured theirs. But their *ſaccharum* was not formed from the expreſſed juice, as is the caſe with that which we uſe at preſent, but was collected, in the manner of a gum, upon the reeds themſelves. This circumſtance, however, may even be called in queſtion by thoſe who wiſh to ſearch only for truth, and to ſeparate what is certain from what is doubtful. For Galen himſelf

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tum gummium modo, candidum, dentibus fragile, ampliſſimum nucis avellanæ magnitudine, ad medicinæ tantum uſum.

Dioſcorides de Medica Materia, lib. ii. c. 104, edit. Saraceni, p. 112:— Καλειται δε τι και σακχαρον, ειδος ον μελιτος εν Ινδιᾳ ϖεπηγοτος και τῃ Ευδαιμονι Αραβιᾳ, ευρισκομενον επι των καλαμων, ὁμοιον τῃ συστασει ἁλσι, και θραυομενον ὑπο τοις οδουσι καθαπερ οἱ ἁλες. Eſt et quoddam mellis concreti genus, quod ſaccharum nominatur, quodque in India et Felici Arabia in arundinibus reperitur, conſiſtentia ſali ſimile, ac dentibus etiam ſalis modo fragile.

Galenus de Simpl. Medic. Facultat. lib. vii. c. 9. edit. Charterii, vol. xiii. p. 207: Και το σακχαρ καλουμενον ὁπερ εξ Ινδιας τε και της Ευδαιμονος Αραβιας κομιζεται. Περιπηγνυται μεν ὡς φασι, καλαμοις, εστι δε τι και αυτο μελιτος ειδος. ἡττον μεν ουν εστιν, η το ϖαρ ἡμιν γλυκυ. Sed et ſaccharum, ut vocant, quod ex India atque Felici Arabia convehitur, concreſcit quidem, ut aiunt, circa calamos, et ipſum mellis eſt ſpecies minus certe noſtrate dulcis, ſed aſſimiles ei vires obtinens, &c.

Arriani Periplus Maris Erythræi, edit. Stuckii, p. 5: Εξαρτιζεται δε συνηθως, και απο των εσω τοπων της Αριακης και Βαρυγαζων, εις τα αυτα τα ϖεραν εμπορια, γενη ϖροχωρουντα απο των τοπων σιτος.και ορυζα——και μελι το καλαμινον, το λεγομενον σακχαρι. Ex intimis locis Ariaces et Barygazorum, in hæc eadem ulteriora emporia ex more ſolito devehuntur variæ res, ut frumentum oryza—et mel arundineum, quod ſaccharum dicitur.

* The celebrated Plaz, in *Program. de Saccharo*, Lipſiæ 1763, adduces the opinion of Reiſke reſpecting the name *ſuccar*, viz. that it is of Perſian or Indian and not Arabic extraction; and that it was brought, together with the thing ſignified, from India to Arabia.

ſeems to have entertained ſome degree of doubt when he ſays,—"it forms itſelf into concretions, *as we are told*, around the reeds." This account alſo may be rendered ſtill more ſuſpicious when we reflect upon the many ridiculous things which both the antients and moderns have related reſpecting articles brought from diſtant countries by merchants, who, for the moſt part, are intereſted in concealing the real nature of their merchandize under falſe accounts. Nor do I ſee any arguments to confute thoſe who might aſſert that Pliny and Dioſcorides were deceived by merchants; ſo that theſe two naturaliſts believed the *ſaccharum*, which was an expreſſed juice, condenſed by art and the help of fire, to be a certain kind of gum. This doubt I, indeed, cannot remove; but let us try whether we can produce any thing ſatisfactory by adopting Pliny's opinion, that the antient *ſaccharum* was the work of nature, and not of art. I ſhall therefore inquire whether our ſugar-cane ever produces ſugar ſpontaneouſly; which, indeed, ſeems not at all improbable, ſince we have camphor both in a natural ſtate and produced by art. This queſtion, indeed, may be determined by the teſtimony of thoſe who in India have made this an object of their attention.

Hernandez, in a book which he wrote on the plants of Mexico[*], relates that he had heard that fragments of ſugar were found ſometimes adhering to the tops of the canes. Matthiolus[†] alſo ſays, that he had heard from men worthy of credit, who had viſited the iſland of St. Thomas and Madeira, that in thoſe iſlands the ſugar is found in a concrete ſtate, like gum, on the ſugar canes, by the exuding of the juice, and not unlike that made by art called *ſugar candy*. John Langius[‡] ſaw alſo ſugar canes, brought from

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[*] *Hernandez Theſaurus Novæ Hiſpaniæ:* Romæ 1651, fol. lib. iv. c. 14. p. 111:—Audio naſci ſponte ſua apud Argentarium, quod vocant, flumen arundines ſacchari, arborum magnitudine, quibus (quod dixerat Plinius in veteri quoque orbe evenire) vi ſolis expreſſi ſacchari orbiculi ſuprema parte adhæreſcant.

[†] *Matthioli Opera*, edita à C. Bauhino: Baſiliæ 1674, fol. epiſtol. lib. i. p. 76:—Sed an quid pluribus de his diſputandum, cum multorum teſtimonio jam notum ſit in inſulis, quas Canarias et Divi Thomæ appellant, in iiſdem ſane calamis, a quibus ſaccharum exprimunt, illud etiam per ſe gigni, et aperto arundinis latere, manare, et gummi modo coaleſcere: quibus facile ſubſcribunt ii, qui hoc ævo in Indiam navigant. Etenim ad Bethecalam Indiæ utrumque jam vendi affirmant.

[‡] *I. Langii Epiſtolarum Medicinalium Volumen tripartitum*, Hannoviæ 1605, 8vo. p. 320: Vidi quoque ex Madera in Hiſpaniam allatas arundines quibus ſaccharum purum accreverat; quod ſeparatum, ne reliqui decoctio vileſcat, non niſi magni veneunt.

Madeira to Spain, to which ſugar was adhering in a pure concrete form. Amatus Luſitanus[*] writes, that the juice ſometimes exudes from the ſugar cane and concretes in the ſame manner as reſin or gum, and not unlike lumps of ſugar candy; and that the ſame thing happens not only in India, but in Madeira, the iſland of St. Thomas, and the Canaries. Bauhin[†] likewiſe tells us, that there are ſometimes found in that part of the cane called by modern botaniſts the *eyes of the leaves*, grains or tears of ſugar, of the ſize of peas, reduced to a concrete ſtate by the heat of the ſun, ſometimes pellucid, and ſometimes candied and not pellucid. John Coſtæus[‡] alſo believes, from works publiſhed in India, that the ſugar is produced ſpontaneouſly on the canes at Bethecala in that country.

That no doubt may ariſe on account of the ſize of the concretions, which Pliny ſays were ſometimes as large as a walnut, I ſhall here obſerve that this might be eaſily accounted for, ſince the drops adhering to the leaves, being melted by the heat of the ſun, might fall down and be united into one maſs. Manna concreted in this manner was ſeen by Niebuhr in Arabia[§].

We have therefore a ſufficient number of teſtimonies which ſeem to confirm the account given by the antients of their *ſaccharum*. I am acquainted, however, with no later authors who ſaw ſugar in that form; and Rumph[||] denies that any of this kind is to be found in India: but that author, who examined with great attention the nature of the ſugar cane, aſſigns a reaſon, not at all improbable, why ſugar is never now formed ſpontaneouſly on the canes. The planters, he ſays, do not ſuffer the canes to attain to a ſufficient age for the exudations to be produced, becauſe the cane every year becomes more woody, and loſes its beſt juice. Beſides, he thinks it probable that the juice might exude from the canes more eaſily and more frequently in the warm regions of Arabia Felix and Indoſtan than in the iſlands of India, which are more expoſed to rains.

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[*] *Amati Luſitani Enarrationes in Dioſcoridis de Medica Materia Libros:* Argentor. 1554, 4to. p. 227:—Veteres ſaccharum dixerunt ſal in arundinibus collectum; nec ab re, cum in ipſis arundinibus non quidem fiſtuloſis, ſed ſucco plenis, magno æſtu. ſuccus foris attrahitur, et ibidem ac reſina vel gummi in ipſis arundinibus denſatur ac concreſcit, ſimilis omnino ſaccharo cando dicto, hodie arte parato, vel ſali dulci, unde multi illum ſalem Indicum appellabant. Nec tantum in India hoc evenire novimus; ſed etiam hodie in inſulis Maderæ et S. Thomæ ac Fortunatis quas Canarias dictitant.

[†] Caſp. Bauhini Theatrum Botanicum: *Baſiliæ* 1658, fol. p. 298.

[‡] *Coſtai Annotat. ad Meſuæ Libr.* 1. *Secret. Med. Diſtinct.* 1. in initio: Saccharum antiquorum ex eadem planta provenire, ex qua noſtrum ſaccharum vulgare, diſces ex Indiæ commentariis; adhuc enim apud Bethecalam Indiæ tale videre eſt, iiſdem arundinibus ſponte adnaſcens, ex quibus reliquum purius atque impurius conficitur.

[§] Beſchreibung von Arabien: *Copenhagen* 1772, p. 146.

[||] Rumphii Herbarium Amboinenſe, pars v. p. 190.

There are, however, ſtill other cauſes of doubt; for even if we allow that the ſugar cane ſometimes produces concretions, yet it is not probable that they poſſeſs the ſame properties as thoſe which the antients aſcribe to theirs. For example, theſe concrete drops, on account of their melleous nature, ought, according to every appearance, to be rather of a viſcid and ſticky nature than brittle; and this doubt appears to me of more importance than that propoſed by ſome learned men who have compared the medical properties of the antient and modern ſugar. For though ſome aſcribe the ſame qualities to our ſugar as thoſe for which the antient phyſicians extolled theirs, yet many aſſert that there is ſo great a difference, that they believe the two ſubſtances to have been totally different *. But, in my opinion, there muſt neceſſarily be a great difference between freſh ſugar formed ſpontaneouſly, and juice expreſſed by great force, and afterwards inſpiſſated by the help of fire, and purified by the addition of various ſubſtances. Many particles are no doubt deſtroyed by boiling, and many are thrown out with the ſcum, as the juice cannot otherwiſe be formed into a cryſtalline maſs, ſince the particles, which are too mucilaginous and oily, and in the ſeparation of which conſiſts the refining of ſugar, oppoſe cryſtallization too much. It is indeed not poſſible in any other manner to produce our common white ſugar, which is forced to cryſtallize; but the cryſtals are exceedingly ſmall, and, by their coheſion, form a continued maſs. Beſides, all thoſe who have made experiments on the nature of new ſugar found in it all thoſe medical virtues † extolled

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* Dioſcor. lib. c. Εϛι δε ευκοιλιον, ευϛομαχον, διεθεν ὑδατι και ποθεν· ωφελουν κυϛιν κεκακωμενην καὶ νεφρους. καθαιρει δε και τα τας κορας επισκοτουντα επιχριομενον. Eſt ſaccharum alvo idoneum et ſtomacho utile, ſi aqua dilutum bibatur; vexatæ vero veſicæ renibuſque auxiliatur; quin et illitum ea diſcutit quæ pupillis tenebras offundunt.

Galenus, lib. c. Saccharum aſſimiles melli vires obtinens, quod ad abſtergendum, deſiccandum et digerendum attinet; porro quatenus nec inimicum ſtomacho eſt, ceu noſtras, nec ſitim afferens, ea tenus ab illo ſubſtantia differt. Conf. *Galeni Methodus Medendi*, lib. viii. c. 4. edit. Charterii, vol. x. p. 191.

† See *Voſſius ad Melam*, p. 271, where may be found the following quotation from Julius Landus:—Arundines incolæ (puto, in inſula S. Thomæ) maturas et recentes comedunt jejuni, voluptatis gratia, ac etiam his de cauſis, ut inquiunt, quod ſolvunt ventrem, jecur refrigerant, ſitim auferunt,

ſo much by the antients. Thoſe therefore who, from the reaſons I have adduced, may be inclined to believe that the ſugar of the antients was the ſame as that now in uſe, will be readily perſuaded that the antient ſugar differed in nothing from ours but in boiling alone, and deſpumation, according to the phraſe employed by chemiſts.

In my opinion, the antients were acquainted with a certain ſweet ſubſtance collected from reeds; but it cannot be proved in a manner ſufficiently ſatisfactory that it was the tears of the ſame cane or plant from which our common ſugar is procured; eſpecially ſince there are many kinds of reeds found in India which exude melleous drops. Leſt this conjecture ſhould appear to be too remote from the truth, I ſhall relate what a jeſuit who had travelled through the ſouthern part of America wrote to Francis Mar. Piccolo: "That in India melleous dew was ſometimes ſo common that it formed itſelf on reeds in great abundance, and might thence be collected with very little trouble. He taſted theſe drops, and found that they were perfectly ſimilar to ſugar in every reſpect except the colour*."

In relating the accounts given by the antients of *ſaccharum*, I purpoſely omitted the words of Alexander of Aphrodiſias for two reaſons: firſt, becauſe they are wanting in all the Greek editions of that author, and are found only in the Latin tranſlations; and therefore it is impoſſible to ſay what his opinion was†. In the ſecond place, I omitted the paſſage

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auferunt, albos etiam faciunt dentes. Nimirum res eſt ſuaviſſima, neque cum ſacchari arundinibus quæ Romæ venundantur, comparanda. Puerperæ etiam utuntur offa ex pane primum torrefacto, deinde in ſucci arundinum ultima coctura poſito. Superaddunt vitellos, quod is cibus amiſſas vires maxime reficiat, ac ſtomachum et inteſtina recreet ventremque paullatim molliat.

[*] *Lettres édifiantes* V. *Recueil*, p. 264: Dans le mois d'Avril de May et de Juin, il tombe avec la roſée une eſpece de manne qui ſe congele et qui s'endurcit ſur les feuilles des roſeaux, ſur leſquelles on la ramaſſe. J'en ai goûté. Elle eſt un peu moins blanche que le ſucre; mais elle en a toute la douceur.—We are told alſo that the ſame thing takes place in the kingdom of Chili. See *Vidaure in Compendio della Storia Geographica Naturale e Civile del Regno del Chile*; Bologna 1776, 8vo. p. 13: and the German tranſlation, Hamburgh 1782, 8vo. p. 23.

[†] *Alexandri Aphrodiſiei Problemata, omnibus Studioſis non minus utilia quam jucunda, Græce et Latine Joannis Davioni Studio illuſtrata:* Pariſiis 1541, 12mo. lib. ii. § 78. p. 70. 6:—Quod ſacchar Indi appellant, mellis congulum eſt, ſole cogente rores, convertenteque ad mellis dulcedinem. Quod idem monte Libani fieri certum eſt. Fit ſacchar id proximum ſalis globulis, candidum, fragile, vim id quoque tergendi purgandique melli ſimilem habet, et, coctum ritu mellis noſtratis, non æque atque incoctum purgare poteſt. Cibo tamen ita aſſumtum aptius ad alendum eſt.—There

ſage here alluded to, becauſe it is evident that Alexander of Aphrodiſias thought *ſaccharum* to be the ſame as that ſubſtance called at preſent *manna*; for he ſays, *Mellis coagulum eſt quod ſacchar Indi appellant quod idem in monte Libani fieri certum eſt.* But Libanon produces fruit-trees, and not canes that yield ſugar. And we are told by Galen, that ſuch abundance of honey was found on the leaves of the trees and ſhrubs of Mount Libanon, that the huſbandmen uſed to ſing, "Jupiter rains honey *." Alexander of Aphrodiſias, therefore, alluded to manna, which he called *ſaccharum*, or *ſacchar*; but manna, though it has an affinity to ſugar, is a ſubſtance totally different.

But it is poſſible that the antients may have been acquainted with ſugar, to which they gave ſome other name; and there are even learned men who think that ſugar is to be underſtood in various paſſages of the Greek and Roman authors, where they ſpeak of honey procured from reeds, of Indian ſalt, and of a ſweet liquor obtained from the roots of canes. There can be no doubt that the antients conſidered their *ſaccharum* as a kind of honey. Whatever was ſweet, the antients called honey; and for this reaſon, latter ages beſtowed the ſame appellation on every thing endowed with peculiar ſweetneſs. In this manner, manna is called *aëromeli* by Athenæus †, or rather Amyntas: beſides, from manna diluted with water in the ſame manner as honey, they made a kind of beverage which they praiſed as being ſweeter than honey. No wonder then if they thought *ſaccharum*, which was found in ſpontaneous concretions upon reeds, and collected in that form, to be, as well as manna,

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is preſerved in the library of our univerſity a very ſcarce French tranſlation, entitled, *Les Problemes d'Alexandre Aphrodiſé—traduits de Grec en Francois* par M. Heret, à Paris 1555, 8vo. where the preceding paſſage is thus tranſlated, p. 93:—Ce qu'on appelle ſucre d'Inde eſt un lopin de miel fait par le ſoleil coagulant la roſée, puis la convertiſt en douceur de miel. Ce que certainement ſe fait au mont de Liban. Ce ſucre premierement eſt blanchi, puis devient friable, et preſque ſemblable aux motes de ſel. Auſſi ha il puiſſance de nettoier et purger ſemblable au miel. Mais s'il eſt cuit comme le miel de noſtre païs il ne peut purger ainſi que s'il n'eſtoit cuit; toutefois ſi on le mange en ceſte façon, il eſt plus propre à nourir. Comp. *Fabricii Biblioth. Græca*, vol. iv. p. 74.

* *Galenus de Aliment. Facultat.* III. 38. p. 119. tom. ii. Baſil. 1559:—Memini me aliquando cum ætate ſuper arborum ac fruticum herbarumque folia mel quam plurimum fuiſſet repertum, agricolas velut ludentes ceciniſſe Jupiter melle pluit. Apud nos vero raro id contingit, in monte autem Libano quotannis perſæpe. Itaque coria ſuper terram extendentes ac arbores excutientes, quod ab eis defluit, excipiunt, et ollas ac fictilia melle implent, vocantque id mel roſcidum et aerium, δροσόμελι καὶ ἀερόμελι.

† Lib. xi. Deipnoſoph. edit. Caſauboni 1597, fol. p. 500.; Theophraſti Opera, edit. Heinſii: *Lugduni Bat.* 1613, fol. p. 474.

a kind of honey. But when I conſider every thing that the antients have ſaid of honey found on reeds, I can diſcover no certain arguments by which it can be proved that it was the ſame with our ſugar.

Among the fragments of Theophraſtus, there is one preſerved which treats of honey. That author, as he generally does, has diſtinguiſhed with more care than other authors all the kinds of ſweet ſubſtances known in his time. He, however, comprehends them all under the appellation of honey. "The firſt (ſays he) is that which is made on flowers; the ſecond is that which falls from the atmoſphere upon plants of any kind; and a third is formed upon reeds." The firſt kind which the author here ſpeaks of, is, without doubt, that honey extracted by bees from flowers; the ſecond is a melleous dew, which, no doubt, belongs to the manna of the moderns; and the third ſeems to be the *ſaccharum* of Pliny and the Greeks, of which I have hitherto been ſpeaking. But nothing can be drawn from this paſſage to elucidate the hiſtory of our ſugar *. Megaſthenes ſays, according to Strabo, lib. xv. that honey is produced in India on reeds without the labour of bees. The words of Iſidore † convey as little information: "Honey formerly was a kind of dew, and was found on the leaves of reeds; and hence Virgil ſays, *Hactenus aërii mellis celeſtia dona.* Even yet, indeed, it is found in a concrete form, like ſalt, adhering to branches of ſhrubs in India and Arabia." Seneca expreſſes himſelf almoſt in the ſame manner:—"It is ſaid that honey is found in India on the leaves of reeds, and that it is produced either by the dew of heaven or by the ſweet juice of the reeds themſelves. Our herbs alſo have a property of the like kind, and the honey is ſought for and collected by inſects ſuited by nature for that purpoſe ‡," &c.—Theſe words appear to me to be at any rate worthy of notice, as they confirm what I have already aſſerted, that the origin of *ſaccharum* was little known to the Greeks or the Romans, ſince Galen and Seneca ſay expreſsly that they related merely on report a circumſtance which was therefore uncertain and leſs probable.

As Dioſcorides compares the *ſaccharum* found on reeds to ſalt, and Paulus Ægineta § makes mention of *Indian ſalt*, which differed neither in colour nor ſubſtance from common ſalt, and which was ſaid to be endowed with a melleous taſte, many men of learning have been induced to believe that this ſalt is the ſame as that called *ſaccharum* by Dioſcorides, Pliny, and others. But, even if we allow this to be true, we learn nothing from it that can illuſtrate Pliny's account of *ſaccharum*; for the Indian ſalt is recommended by Paulus Ægineta only to thoſe whoſe tongues are rough and foul, to whom I do not know whether any ſweetneſs can be of ſervice, whether procured from reeds or from any other plant.

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* Boſe, formerly profeſſor, agrees with me in this opinion. See his *Otia Wittembergenſia Critico-phyſica, Diſſertat. hab.* 1739, p. 16.

† Origines, lib. xx.

‡ Epiſt. 84. edit. *Lipſii*, p. 549.

§ *Pauli Æginetæ Opus divinum, Albano Torino Vitodurenſi Interprete,* Baſileæ 1532, fol. lib. ii. c. 54, p. 95: Speaking of a foulneſs of the tongue, he

It now remains that I ſhould conſider that ſweet liquor which the Indians are ſaid to have made from the roots of reeds. This beverage is mentioned by various authors both Greek and Latin. Solinus, indeed, tells us that this reed was of a prodigious ſize; but Voſſius very properly obſerves, that the ſize which he aſcribes to the Indian reeds or canes could not be that of our ſugar cane. It is therefore probable that Solinus either committed a miſtake in aſcribing to theſe large canes a juice which in reality was expreſſed from a much ſmaller kind of reeds, or that the liquor alluded to was the produce of a plant quite different from that which produces ſugar. But ſome verſes of Varro extolled by Iſidore *, and

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he ſays—Archigenes et ſal Indicum huic uſui adhibuit, quod ſane colore et ſubſtantia nihil a communi ſale diſſidet, ſapore vero meliteum eſt; lentis vero ejus ſalis magnitudo, vel ad ſummum fabæ quantitas comeſta, affatim atque profuſe humectare prodeſt.—The ſame author, lib. vii. p. 388, ſays: Quin etiam ſaccar quod vocant mel, quod ex Felici Arabia invehitur, non tam dulce quam noſtras eſt, ſed æquales ei vires poſſidet, neque ſtomacho moleſtum eſt, neque ſitim conciliat. *Avicenna*, fen. i. can. 4. tract. 2. c. 231. ſpeaking of foulneſs of the tongue in fevers, follows Paulus Ægineta: Aut teneat in ore ſalem qui aſportatur de India, et eſt in colore ſalis, et dulcedine mellis; et ſumat de eo, ſecundum quod dixit Archigenes, quantitatem fabæ unius.

* *Iſidor. Origin.* lib. xvii. c. 7: In Indicis naſci arundines calamique dicuntur, ex quorum radicibus expreſſum ſuccum bibunt, unde et Varro ait:

Indica non magna nimis arbore creſcit arundo:
Illius e lentis premitur radicibus humor,
Dulcia cui nequeunt ſucco concedere mella.

Nobody acquainted with Latin will deny, ſays Voſſius, that there is ſomething here in the firſt line which muſt offend every ear not vitiated. Saumaiſe increaſes inſtead of curing the fault, when he reads *Indica nam magna*. Voſſius thinks that the reading ought to be:

Indica non magna minus arbore creſcit arundo.

To the ſame ſubject belongs the following line of Lucan, Pharſal. iii. p. 237:

Quique bibunt tenera dulces ab arundine ſuccos.

I ſhall ſubjoin alſo the words of Pomponius Mela, lib. 3. c. 7:—Tam pinguis and ſome expreſſions alſo of Strabo *, ſeem to prove that the Indian reeds were of an immenſe ſize; though ſome doubt may be entertained reſpecting the opinion of Varro, as the words are ſaid to vary in different editions of the book. Nor can there be any doubt but that there are many kinds of reeds from which a ſweet juice may be expreſſed equal in ſweetneſs to honey. But the principal reaſon why I cannot aſſent to the opinion of thoſe who believe that the liquor of the Indians was made from our ſugar cane is, that no recent author has praiſed the melleous juice of the roots, which are ſlender and not very ſucculent. Nor did Rumph ever hear in India, that a ſweet and potable juice was expreſſed there from the roots of either reeds or trees. The antient Indians, had they known our plant, would, without doubt, have expreſſed from the trunk or ſtem a potable liquor, as the narrow or ſpongy part is ſo full of juice that it may be ſucked out with great eaſe. But I do not ſee how thoſe could be refuted who might aſſert that the antients were deceived in this point by travellers, and falſely imagined that the juice expreſſed from the ſtem was contained in the roots. Nay, this opinion might be confirmed by this circumſtance, that Rumph himſelf, who certainly examined and was well acquainted with every thing relating to the nature of the ſugar cane, its form and ſize, imagined that the tops of the canes, which for the moſt part incline towards the ground, might have been conſidered by the antients as roots †. And this opinion acquires ſome weight when it is known that the part of the ſtem neareſt to the root or the earth contains the largeſt quantity of juice, as appears from the account of Rauwolf ‡ and others.

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pinguis alicubi et tam feracis ſoli, ut in ea mella frondibus deffluant, lanas ſilvæ ferant, arundinum fiſſa internodia veluti nevia, binos et quædam ternos etiam vehant.

* Strabo, lib. xv. ex Eratoſthene: *Τους δε ξυλινους καρπους, γενασθαι πολλους, και τας ῥιζας των φυτων και μαλιστα των μεγαλων καλαμων γλυκειας και φυσει και ἑψησει χλιαινομενου του ὑδατος τοις ἡλιοις, τουτ' ἐκπιπτοντος ἐκ Διος, και του ποταμιου.* Fructus autem arboreos multos gigni, radices ſtirpium, præſertim magnarum arundinum et natura et decoctione dulces eſſe, aqua a ſolibus tepefacta, tam cæleſti, quam pluviali.—Thoſe deſirous of being acquainted with the various kinds of reeds known to the antients may conſult *Theophraſt. Bodæi a Stapel.* p. 480.

† Herbarium Amboinenſe, vol. v. p. 188.

‡ *Beſchreibung der Reiſe Leonh. Rauwolffen*; Frank. 1582, 4to. lib. I. c. 4. p. 53. The author, ſpeaking of Syria, ſays: "Sugar canes grow exceedingly well here, and in other places in the neighbourhood. The canes in ſize and height are not unlike our canes, but the interior part of them, and particularly at the bottom near the roots, where they have moſt

ſtrength,

This eſſay might appear very defective if I paſſed over in ſilence the opinion of Saumaiſe[*] reſpecting the ſugar of the antients, which has been adopted by many learned men. This author endeavours to prove, by a variety of arguments, that the antient *ſaccharum* was the ſame as the modern *ſacchar mambu* of the Indians, and the *tabaxir* of the Perſians and the Arabs. I ſhall not, however, adduce all the proofs which he employs, and which have been frequently quoted by others, but confine myſelf to a few obſervations. As I never had an opportunity of ſeeing, as far as I remember, the ſugar called *mambu*, as none of it has been imported into Europe in my time, we muſt conſider it to be that mentioned by thoſe who have been in India, and who have often ſeen *tabaxir*.

The tree which produces it, and which is very common in India, is called by botaniſts *arundo bambus*[†]. Between its joints a ſweet thick juice exudes, ſometimes in a large and ſometimes a ſmaller quantity. Every tree of this ſort does not, however, exude that liquor; but thoſe only which grow in Biſnaga, Betecala, and part of the province called Malabar; and for this reaſon many who ſpeak of this tree, or reed, ſay nothing of its ſugar. This liquor, when found in a concrete ſtate, is ſometimes blackiſh and ſometimes of an aſh colour, but on that account is not conſidered to be of a worſe quality. According to the teſtimony of Indian, Arabian, Perſian, and Turkiſh phyſicians, the *tabaxir* is of ſervice in external and internal heats, and alſo fevers and dyſenteries. In the time of Garcias ab Orto[‡], whoſe words I have hitherto uſed, this *tabaxir* was as dear as ſilver. The name *ſacchar mambu* ſignifies the ſame as ſugar of *mambu*, as the Indians give the name of *mambu* to the reeds or branches of the tree that produces it. Piſo[§] derives the word *tabaxir* or *tabarzad*, for ſo it is read in Rhaſes, who lived in the

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ſtrength, they are full of an agreeable excellent juice, and for this reaſon are purchaſed at a dear rate by the Turks. Before they begin to uſe theſe canes, they ſtrip off the long leaves and cut away the dry part at the top, ſo that the remaining part, which is good, and full of ſap, is ſcarcely an ell in length. Some of them carry about with them through the ſtreets canes prepared in this manner as our boors do pipes, and cutting off bit after bit, and peeling away the rind, chew them publicly with great pleaſure; for at the bottom near the root they are very ſappy, and as tender between the teeth as a lump of ſugar."

[*] Salmaſ. ad Solinum, p. 719.

[†] Pomet Aufrichtiger Materialiſt, *Leipzig* 1717, p. 114.

[‡] Aromatum et Simplicium Medicamentorum Hiſtoria, *Antwerpiæ* 1574. p. 49.

[§] Plaz de Saccharo, p. 5.

ninth century, from the Perſian language, and explains it by the expreſſion (*lac lapideſcens*), petrified milk; but Garcias calls it (*humorem lacteum*), milky juice.

I ſhall now proceed to the opinion of Saumaiſe, which I allow to be extremely probable. Tabaxir is a juice which ſpontaneouſly concretes in the manner of gum on very tall reeds; it is ſaid to be ſweet, and, like the *ſaccharum* of Pliny, to be uſeful only in medicine. The name *ſacchar* ſeems to have been given by the Indians ſince the remoteſt periods to the juice of this reed, and latter ages have beſtowed the ſame appellation on the juice of our ſugar cane, becauſe, when condenſed by the help of fire, it approaches very near in appearance to the ſugar of the *mambu*. Some of the young reeds of the *mambu* are ſo ſpongy and abundant in juice, that they are chewed with great avidity by the common people on account of their agreeable taſte. But ſome very weighty reaſons prevent me from agreeing in opinion with Saumaiſe: for, in the firſt place, that the reed called *bambu* is a tree of ſuch a ſize that canoes may be formed of it, is no argument; for it can by no means be proved that the reed of which Solinus and Dioſcorides ſpeak, is the ſame plant from which they collected their *ſaccharum*: nay, Solinus ſays in expreſs words that the Indians ſqueezed from the roots of this reed a potable juice. But more modern botaniſts and travellers, whoſe veracity is unqueſtionable, and who were well acquainted with the tree, deny that any ſweet and potable juice can be expreſſed from its roots. Such of the antients as have ſpoken of the Indian reeds, and preſſing juice from them, have never mentioned *ſaccharum*: on the other hand, thoſe who expreſsly mention *ſaccharum* do not ſay a ſingle word of ſqueezing juice from it. Beſides, though the juice of the young plants be of a ſweet and agreeable taſte, yet the tears which exude near the joints, and are condenſed by the heat of the ſun into the form of white pumice ſtone (for ſuch it appeared at leaſt to Piſo), have no ſweetneſs at all, or rather a peculiar taſte a little aſtringent, and ſimilar to that of burnt ivory [*]. For this reaſon Rumph, who often taſted *tabaxir* in India, aſſerts that he found no ſweetneſs in it, and that it approaches much nearer to amyli than to ſugar or ſalt.

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[*] *Gul. Piſonis Mantiſſa Aromatica, c.* 10. *quæ adjuncta eſt Piſonis Libris de Indiæ utriuſque Re Naturali et Medica*; Amſtelodami 1658. fol. p. 186: Arundines mambu adoleſcentes medulla levi, ſpongioſa et liquida (non adeo farcta ut vulgares cannæ ſacchariferæ) refertæ ſunt, quam vulgus grati ſaporis gratia avide exſugit. At ubi arundines hæ proceræ et annoſæ factæ fuerint, liquoris contenti ſubſtantia, color, ſapor, et efficacia mutatur (ſicut in nuce coco adulta etiam fieri videamus), atque paulatim protruditur foras, et juxta internodia vi ſolis coagulatur.

In the laſt place, whatever kind of ſubſtance the *ſaccharum* of the antients may have been, it was not well known either to the Greeks or the Romans. It was not in common uſe, and employed only in medicine. Nor do we read of any other ſweet ſubſtance being uſed for ſeaſoning food, making ſweetmeats, and preſerving fruit, than honey, in which apples of all kinds and even fleſh were preſerved [*]. The antient phyſicians alſo mixed honey with their medicines in order to render them more palatable. But even this circumſtance ſeems ſufficient to prove that they were not acquainted with our ſugar, which without doubt might have been employed much more conveniently both for food and in medicine. It is true, indeed, that many things uſed for ſeaſoning were employed firſt in medicine, and were then, after a long interval, introduced into cookery. Thus, ſpirit of wine and grain were long recommended for the uſe of medicine under the name of *aqua-vitæ*; in the like manner, coffee, tobacco, and many other things that now miniſter to the luxury of our tables, or are in common uſe, were firſt known as medicines. Even oranges were introduced into cookery only at a late period among the Romans, and were not commonly eaten. Oranges, ſaid Pliny [†], are not eaten but as a remedy for poiſon; and they were put into wardrobes with clothes in order to give the latter an agreeable ſmell [‡]. Nor does the uſe of oranges, which in modern times are ſo neceſſary in food, that the art of cookery would be at a loſs could they not be procured, occur as ſeaſoning in the work of Apicius. We have reaſon therefore to conjecture that *ſaccharum* was known and uſed as a medicine, but not as food. But as the antients prepared a beverage for which they before uſed common honey or aërial honey, called at preſent *manna* [§], why ſhould they not have tried *ſaccharum* in the ſame manner? for though they had not *ſaccharum* altogether purified, yet it is evident that the taſte of even impure ſugar would have been more agreeable than that of the above nauſeous manna. Nor is it probable that the antients, had they known ſugar, would have abſtained from it on account of its dearneſs, when they ſpared nothing in their ſeaſonings that could gratify the organs of taſte. Pepper, for example, was ſold in large quantities at more than its weight in gold or ſilver*: nevertheleſs, it was ſo much eſteemed that it was the moſt common of all ſpices, and it is ſcarcely ever omitted by Apicius in ſix hundred diſhes.—Our ſugar was therefore unknown to the antient Greeks and Romans.

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[*] *Columella*, lib. xii. c. 10: Illud in totum præcipiendum exiſtimavi, nullum eſſe genus pomi quod non poſſit melli ſervari; itaque cum ſit hæc res interdum ægrotantibus ſalutaris cenſeo, &c. See alſo *Apicii* lib. i. c. 8: Ut carnes ſine ſale quovis tempore ſint recentes.

[†] Plin. Hiſt. Nat. lib. xii. c. 3.

[‡] Athenæus Deipnoſ. lib. iii. c. 7.

[§] Athenæus, lib. xi. p. 500.

II. *Deſcription of a Dwarf of a very remarkable Conformation. By Baron* F. VON WURMB, *late Secretary to the Batavian Society of the Arts and Sciences in the Iſland of Java*†.

A FEW years ago there was ſeen at Batavia a native of the iſland of Bali ‡, named Kitip, who was ſo ſmall, and at the ſame time diminiſhed in ſo ſymmetric a manner, that he deſerves to be conſidered as one of the rareſt phænomena in the animal œconomy of nature.

His whole length amounted only to two feet eleven inches Engliſh meaſure, or about $33\frac{1}{7}$ Rhinlandiſh inches; and the reduction of his body on both ſides ſo regularly alike, that the opinion of Buffon reſpecting the power of ſimple parts acting on both ſides with equal force, from which ariſe the double parts on the expanſion of the animal body, ſeems to be fully confirmed. The head, though by far too large for the body of this dwarf, and which might have ſuited one of ſix feet, exhibited in other reſpects nothing remarkable. His countenance, conſidering his race, was not diſagreeable, as the features ſeemed to expreſs a grave and not unpleaſant diſpoſition. His hair was long and black, but here and there a little gray, becauſe he had already attained to the fortieth year of his age. Like moſt of his countrymen, he had no beard. His ſkin was of a browniſh colour. His ſhoulders were broad, and his cheſt ſtrong. The belly had the uſual form, only that it was too much contracted towards the lower part. His arms were too long in proportion to the body, and a little twiſted or turned outwards. The lower part of the arms was only half as long as that above the elbow: the hands were ſhort, broad, and of a round form before. The fingers conſiſted of round ſtumps, which were totally looſe, without being ſtrongly connected with the bones of the hand behind, and which were capable of no movement. They were furniſhed with nails, and conſiſted of the third row only of the finger joints, as the firſt and ſecond were totally wanting. Each hand had ſix of theſe ſhapeleſs fingers; two ſtanding cloſe together in the place of the little finger, and the other four in the place of the thumb, the fore, middle, and ring finger. The bones of the hand were bent a little outwards, by which means the fiſt appeared round, and the back of it ſomewhat hollow. This ſmall cavity was of great ſervice to little Kitip on account of the ſhortneſs and immobility of his fingers, eſpecially at the time of his meals, as he uſed the back of his hand inſtead of a ſpoon in order to convey rice and other food to his mouth. His legs were remarkably ſhort; the feet ſeemed to have grown out from them quite ſolid; for, though their motion gave reaſon to ſuppoſe the exiſtence of the bone of the leg and iſchia, there was, however, no external appearance of them. The knees were entirely wanting. The feet were ſhort and broad. Each foot had ſix toes ſimilar in form to the fingers, and which had grown out in the like manner—but with this difference, that they were connected with the feet by a ſmall part of their round ſurface, and that the two which ſtood in the place of the little toe adhered ſomewhat to each other.

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* Plin. Hiſt. Nat. lib. xii. c. 7.

† From the Tranſactions of the above Society.

‡ Bali or Balei is one of the ſmall iſlands lying on the eaſt coaſt of Java, the inhabitants of which carry on a trade in ſlaves with Batavia, and procure in return ſuch articles as they ſtand in need of. This iſland is called alſo Leſſer Java.

This dwarf was born of well proportioned parents at Squoati, in the iſland of Bali, in the year 1740. He never had the ſmalleſt intercourſe with the female ſex, nor ſhowed the leaſt inclination for it. It appeared, however, that he was far from having been neglected by nature in this reſpect, and that his continence aroſe rather from an opinion he entertained, that if he denied himſelf the gratification of intercourſe with the ſex in this world, he would obtain a more perfect body in the next, but that on this ſide the grave he would remain a deformed dwarf. However much he was convinced that Nature had neglected him at his birth, he nevertheleſs believed ſhe had given herſelf the trouble to preſerve him by a miracle, which took place ſoon after he was born. He related, in this reſpect, that his mother, aſhamed of having brought ſo deformed a child into the world, reſolved immediately to deſtroy him, but found to her aſtoniſhment, when ſhe attempted to commit this barbarous deed, that her child was completely invulnerable. This miraculous circumſtance filled her with aſtoniſhment, and conducted her back to parental duty.

The following are the proportions of this dwarf, according to meaſurement:—The whole length 2 feet 11 inches. From the ſoles of the feet to the commencement of the thighs $9\frac{1}{2}$ inches. Length of the foot 3 inches, height of the foot $2\frac{1}{2}$ inches: breadth of the foot before 3 inches. Length of the arm 10 inches. From the elbow to the joint of the wriſt 5 inches; from the joint of the wriſt to the tip of the middle finger 2 inches. Length and circumference of the finger of an inch. Length and circumference of the thumb 1 inch. Diſtance from the tip of the thumb to the tip of the little finger 3 inches. Breadth of the ſhoulders 1 foot. Length of the head 9 inches. Circumference of the upper part of the arm 9 inches; circumference of the wriſt 5 inches; of the neck 12 inches; of the body at the nipples of the breaſt 2 feet 6 inches; of the body at the navel 2 feet 2 inches; of the thigh 1 foot 7 inches; of the ancle 9 inches.

III. *Directions for Tanning all Sorts of Hides and Skin, according to the new Proceſs introduced by Mr.* W. DESMOND. *Communicated by the Author.*

SIR, New Palace Yard, Sept. 26.

I AM flattered to find that you approve of the *Directions for Tanning*; it is certainly my wiſh that this proceſs ſhould be as generally known and practiſed as poſſible. The extent and ſucceſs with which it is and has been carried on during the laſt three or four years by ſome of the principal tanners in Warwickſhire, Staffordſhire, and other neighbouring counties, may be ſufficient inducements to all tanners to make the experiment, and convince themſelves that leather, equal if not ſuperior in quality to the beſt hitherto known, may be manufactured in about as many days as it generally requires months to tan it well in the uſual way, and with leſs expenſe of labour.

Tanners in beginning the new proceſs are apt to be ſtartled at the quantity of bark which it *ſeems* to require; but a little reflection and experience ſoon ſatisfy them that, whatever mode they adopt, the ſame quantity of bark is neceſſary to tan a pound or any given weight of leather, and that the bark is much leſs liable to be waſted or loſt in the new than in the ſlovenly old mode: they alſo find that, by proper management, they can give their hides an additional weight, equal upon an average to ten per cent. upon each more than in the uſual way.

A hide, to be properly tanned, muſt have imbibed the quantity of *tanning* neceſſary to ſaturate it; and whether it receive that quantity in one or in eighteen months, it can make no difference in the quality of the leather, but a very material one to the manufacturer.

The opinion that ſo many months were neceſſary to tan hides, was founded on the clumſy and injudicious mode of throwing the hides and bark together in the pits; but this opinion is now rapidly giving way, and probably will, with your aſſiſtance and that of other ſcientific men, be totally exploded in leſs time than was hitherto required to tan a hide. Tanners will be much indebted to you for your endeavours to undeceive them; and the public, I think, muſt be ultimately benefited by the general adoption of this proceſs.

I am very reſpectfully,
Sir,
Your humble and obedient ſervant,
Alexander Tilloch, Eſq. WILLIAM DESMOND.

Directions for Tanning.

AS that part of the preparation of hides and ſkins, properly called tanning, conſiſts in impregnating and ſaturating them with a principle obtained from tan, by which they acquire ſtrength and firmneſs, preſerve a ſufficient flexibility, and become indiſſoluble and incorruptible in water either hot or cold; ſo the nature of this proceſs is, 1ſt, To aſcertain, by a ſimple and certain criterion, ſuch ſubſtances as contain the ſaid principle: 2dly, To extract it from them, to ſeparate it from every other principle by which its effect might be impeded or counteracted, and to give it the degree of ſtrength neceſſary for the intended purpoſe: 3dly, To diſpoſe the hides and ſkins for the introduction of this principle: and, 4thly, To impregnate and ſaturate them completely with the ſame in leſs than a tenth part of the time uſually employed in tanning; all which operations are performed in the following manner:

Provide five veſſels, called *digeſters,* of any convenient materials and dimenſions, with an aperture at the bottom of each: let them be placed near each other, and elevated upon ſtillages or otherwiſe, ſo that a pail or other ſmaller veſſel may be placed under them. Fill the digeſters with tan, that is to ſay, with the bark of certain trees, of oak for inſtance, cut ſmall, bruiſed, or ground to a coarſe powder. Pour water on the tan in the firſt digeſter, where it may ſtand ſome time, or be drawn off immediately. This liquor is to be poured on the tan in the ſecond digeſter, and drawn off as before; then on the tan in the third digeſter, and ſo on until it comes through the tan in the fifth and laſt. The liquor is then highly coloured, and marks from ſix to eight degrees on the hydrometer for ſalts. This liquor is to be uſed for tanning the thickeſt hides; and may, for want of a more characteriſtic name, be called the *tanning lixivium.* It has this peculiar property, that if you take a ſmall quantity of it in a glaſs, and pour on it a few drops of a ſolution of animal glue, the liquor, which before was clear, becomes turbid, and a whitiſh ſubſtance falls to the bottom of the glaſs. The precipitate thus obtained, by means of the ſolution of glue, is a ſure indication that the liquor contains the tanning principle; for this reaſon, that glue being of the ſame nature with the ſkins or hides of which it is made, whatever ſubſtance unites itſelf indiſſolubly with the former, will do ſo likewiſe with the latter.

This ſolution is made by diſſolving a little common glue in water over a moderate fire: by means of it, not only oak bark, but alſo the bark of ſeveral other trees, ſuch as the plane-tree, cheſtnut-tree, the American hemlock-tree, poplar, elm, willow, &c. as well as divers ſhrubs and plants, ſuch as myrtle, ſhumach, &c. all which I call *tan,* are found to contain the tanning principle; and by employing the ſolution as above, it will, in all caſes, be eaſy to aſcertain whether any given ſubſtance contains this principle or not.

In the courſe of theſe lixiviations two things will be obſerved. 1. The liquor running from the firſt digeſter, at length loſes its colour. If in this ſtate you take a little of it in a glaſs, and repeat the former experiment, the liquor no longer becomes turbid, but remains clear; which ſhows it contains no more of the tanning principle: but if you pour into the ſame glaſs a few drops of a ſolution of ſulphate of iron, the liquor becomes thick and black. This liquor is not to be poured on the tan in the ſecond digeſter, but is to be laid by, and uſed for the *depilation,* or taking off the hair or wool, as will be more particularly deſcribed hereafter. It is diſtinguiſhed by the name of *gallic lixivium,* becauſe it appears to contain the ſame principles as galls.

The liquid ſulphate of iron is obtained by diſſolving a ſmall quantity of iron in oil of vitriol diluted with water, or by diſſolving green copperas in water. This ſolution ſerves to aſcertain ſuch ſubſtances as contain the gallic principle. Lime water will alſo produce this effect.

When the liquor ceaſes to grow black by the mixture of the ſulphate of iron, it will be uſeleſs to pour any more water on the tan in the firſt digeſter. This tan, being exhauſted both of the tanning and gallic principles, muſt be removed, and new tan put in its place.

2. You will obſerve that the liquor, after running through all the digeſters, at laſt grows weak. Add to your ſtock of tanning lixivium all the liquor that marks from ſix to eight degrees on the hydrometer. What proceeds afterwards from the laſt digeſter is to be poured on the new tan in the firſt. Then the freſh water is to be conveyed on the tan in the ſecond digeſter, and the liquor of the firſt to be laid by, while it marks ſix or eight degrees on the hydrometer, and added to the tanning lixivium, which muſt always be carefully ſeparated from the gallic. In this manner, the tan in all the digeſters may be renewed, and the lixiviations continued.

The number of theſe lixiviations, as well as the mode of making them, may be varied at pleaſure; the eſſential point is to repeat them ſo as to give the liquor a ſufficient degree of concentration, which may be determined by the hydrometer, and proportioned to the quickneſs required in the operation, and the thickneſs of the hides and ſkins to be tanned; all which experience will ſoon teach. As all kinds of tan are not equally good, it will ſometimes happen that ſix or more filtrations will be neceſſary to obtain a lixivium of ſix or eight degrees: in this caſe, as well as where a greater quantity of liquor is required, the number of digeſters is to be increaſed, and the ſame method purſued as above; and when a weaker lixivium is wanted, three or four filtrations will be ſufficient.

The perſon who directs theſe lixiviations ſhould be provided with the ſolution of glue and ſulphate of iron, already deſcribed, in order to aſcertain the qualities of the different lixivia, as well as with a hydrometer or areometer, properly graduated, to determine their degree of concentration or ſpecific gravity.

Cow Hides, Ox Hides, &c.

1. *Waſhing and Fleſhing.*—They ſhould be waſhed in running water, well cleaned, and fleſhed in the uſual way.

2. *Depilation, or taking off the Hair.*—Immerſe the hides for two or three days in a vat filled with the gallic lixivium, and a mixture of ſulphuric acid, or oil of vitriol, marking 66 degrees on the hydrometer for acids, and in the propor-

tion of 1 to 1000; that is, one part of vitriolic acid to a thouſand parts of gallic lixivium; or 1 pint to 125 gallons. During this immerſion, the hair is detached from the hides in ſuch a manner, that you may eaſily know when they are to be taken out of the vat, that is, when the hair is quite looſe. It is then to be ſcraped off with the round knife on the horſe or beam.

3. *Raiſing.*—When raiſing is neceſſary, which is ſeldom if ever the caſe, immerſe the hides for ten or twelve hours in a vat filled with water, and a five hundredth part of its volume of mineral acid, of the ſame quality as the former, and the operation of raiſing or ſwelling is done.

4. Waſh the hides repeatedly, and uſe the round knife; then they are fully prepared for tanning.

5. *Tanning.*—The remaining part of the proceſs conſiſts in tanning, properly ſo called: for which purpoſe, ſteep the hides for ſome hours in a weak lixivium of only one or two degrees; to obtain which you may take that which runs from the ſecond digeſter, or ſome that has been already uſed for tanning. They are then to be put in a ſtronger lixivium, where in a few days they will be brought to the ſame degree of ſaturation with the liquor in which they are immerſed. The ſtrength of the liquor being then conſiderably diminiſhed, it muſt be renewed; and when the hides are completely ſaturated, that is, fully tanned, which is known by cutting off a bit of the edge, remove the leather, and let it dry ſlowly in a ſhady place.

Calf Skins, Goat Skins, &c.

1. Fleſh them with the knife, and work them in running water, like the others.

2. Steep them in lime water, in which there ſhould be more lime than the water can diſſolve at once. What is not diſſolved will ſubſide to the bottom, but muſt be mixed with the water, by ſtirring it ſeveral times a day.

3. After two or three days, remove the ſkins: when the hair is found quite looſe, ſcrape it off on the horſe; waſh and preſs the ſkins well, until the water running from them is perfectly clear, and the lime totally extracted.

4. Steep them firſt in a weak lixivium, then tan them as above; but obſerve that the tanning lixivium muſt not be near ſo ſtrong as for the hides.

Lime is uſed for theſe ſoft ſkins inſtead of the mixture of gallic lixivium and vitriolic acid, for this reaſon, that the acid always ſwells the leather more or leſs, and becauſe the lime may be more eaſily extracted from them, by waſhing and compreſſing them, than from the thick hides, which when limed are harſh and apt to crack, if the lime is not totally extracted before they are tanned.

Amongſt the different modes of immerſion, which may be practiſed in the courſe of theſe operations, the beſt way ſeems to be that of ſuſpending the hides and ſkins vertically in the lixivia, by means of tranſverſal rods or bars, and at ſuch a diſtance aſunder as not to touch each other in any one point. If they are laid out one over the other, according to the common practice, they will require frequent *handling*, in order that all the parts may be equally ſaturated, and to prevent the folds or plaits that would otherwiſe be formed in them: all this would occaſion a conſiderable loſs of time and labour.

In ſome caſes it will be found expedient to mix freſh tan from time to time with the lixivium; this and other modifications, ſuch as the various ſtrengths of the lixivium, the raiſing or not raiſing the hides, the uſe of the gallic lixivium, &c., which may be found neceſſary, will depend on the ſtate and quality of the hides and ſkins to be tanned, as well as on the purpoſes for which they are intended: all theſe conſiderations muſt be left to the judgment of the manufacturer, but do not in any way alter or change the principle on which this mode of tanning is founded.

Beſides the very great ſavings in point of time and labour, the leather tanned according to the above method, being more completely ſaturated, will be found to weigh heavier, to wear better, and to be leſs ſuſceptible of moiſture, than the leather tanned in the uſual way.

THE following additional explanations may be of uſe to thoſe to whom ſcientific terms and chemical operations are not familiar.

1. Reſpecting the number, the diſpoſition, and the dimenſions of the digeſters. Five or ſix of theſe veſſels are ſufficient to ſhow, by way of experiment, the nature of the proceſs, the different principles contained in the bark, how they are to be diſtinguiſhed and ſeparated from each other, &c. But when the buſineſs is conducted on a permanent and extenſive ſcale, a much greater number will be neceſſary, not only for the purpoſe of ſupplying the required quantity of liquor, but alſo for ſpending or completely exhauſting the tan, particularly in cold weather, or if hard water is uſed, and the bark not ground fine enough. I would recommend twelve digeſters at leaſt, fully to anſwer theſe purpoſes.

They may be diſpoſed in four rows of three veſſels each: the two middle rows placed cloſe to each other; the two ſide rows ſeparated from them by a paſſage for a ſmall cart or wheelbarrow to fill the digeſters occaſionally with new tan, and to remove it when ſpent. The bottoms of the four front digeſters, or thoſe neareſt the vats, ſhould be ſome inches higher than the edge of the vats, ſo that, when the liquor is ready, it may, by means of a ſhoot, and turning the cock in the digeſter, run of itſelf into the vat where it is wanted. Over each of theſe front digeſters is to be placed a ſecond, projecting a little into it, ſo that, by turning a cock, the liquor may run from the upper into the lower digeſter, without the trouble of pumping; and over the ſecond, a third placed in the ſame manner and for the ſame purpoſe.

By this diſpoſition of the digeſters all the labour of pumping is avoided, except from each of the loweſt to the uppermoſt one of the next row. The four inferior digeſters muſt conſequently be provided with an eye each, for the pump to ſtand in when neceſſary.

Beſides the cock or plug placed in the part of the ſuperior digeſters which projects into thoſe immediately under them, there muſt be another in the ſide for the purpoſe of conveying the liquor into the vats, when neceſſary, by means of a ſhoot.

As to the dimenſions of theſe veſſels, their depth ſhould not exceed two feet and a half, or three feet at moſt; but they may be as wide as the breadth of the ſhed or building, where they are erected, will admit; and, if large enough to contain half a ton of bark at a time, the better will they anſwer the deſired end. They ſhould be made of wood, and not pitched. Metallic or brick digeſters would greatly injure the liquor; no cement, or mortar made of lime, ſhould be uſed in them, becauſe lime deſtroys all the tanning principle with which it comes in contact.

Though theſe obſervations are applicable chiefly where new digeſters of the moſt convenient form are to be erected, yet even in old tan yards, where ſome of the pits already in uſe may be converted into digeſters, their number and depth ſhould be attended to. There ſhould be twelve of them, at leaſt, if poſſible, for the reaſons already aſſigned; that is, for ſupplying the neceſſary quantity of liquor, and completely exhauſting the tan: their depth ſhould not exceed three feet, that the preſſure of too high a column of bark may not prevent the water from diſſolving all its parts equally, and that the tan, by clodding, which it is apt to do, may not prevent the liquor from running. No more water ſhould be poured on the tan than is ſufficient to cover it, and therefore the eyes of theſe ſpenders ſhould be no larger than is abſolutely neceſſary for the pump to ſtand in.

It is ſcarcely neceſſary to add, that the bark as well as the infuſions ſhould be carefully protected from rain, ſnow, and hail, which would injure them materially; and even from froſt, if poſſible, becauſe the operation of the liquor when frozen is totally ſuſpended.

2. Five or ſix days are generally ſufficient for the immerſion of the hides in the ſame vats, as by that time they come to an equilibrium, in point of ſaturation, with the liquor; that is to ſay, they acquire all the ſtrength the liquor can give them. They are then to be ſhifted into a ſtronger infuſion, where they may remain the ſame number of days. In mild weather, if the liquor is of a proper ſtrength, three or four immerſions, of five or ſix days each, will be ſufficient to tan the hides, which in the old mode require eighteen or twenty months to be completely tanned. To keep them longer in the ſame liquor would be ſo much time loſt, as in making the infuſions it would be a loſs of time to let the liquor ſtand more than a couple of hours on the ſame tan. It is not the length of time during which the ſame water ſtands on the bark, but the number of waterings, that will completely ſeparate the tanning principle from it. As the heavieſt and beſt part of the liquor always falls to the bottom of the vats, it ſhould be ſtirred up from time to time.

3. The uſe of the ſolution of glue ſhould not be neglected. Since this ſolution has been made known as a teſt for aſcertaining the preſence of the tanning principle in the infuſions of different kinds of bark, a great variety of them have been already found, and many others may yet be diſcovered, to be excellent ſubſtitutes for oak bark; and in proportion as their uſe becomes general it will neceſſarily lower the preſent high price of the latter article, and ſave many young oaks.

Amongſt the barks already found, by means of this teſt, to contain the tanning principle in a greater or leſs proportion, the principal are thoſe of the following trees, viz. willow, aſh, hazel, Spaniſh cheſtnut, poplar, ſallow, cherry-tree, birch, ſycamore, plum-tree, beech, and elder.

4. In tan yards where there is not a ſufficient body of running water for ſoaking and waſhing the ſkins, in order to extract the lime from them, the preſent mode of extracting it in grainers, by means of pigeon's dung, hen's dung, or other alkaline ſubſtances, may be continued; but the ſkins ſhould never be put into the ſame liquor in which hides unhaired with the gallic and vitriolic acids have been tanned. The uſe of theſe acids is not abſolutely neceſſary either to ſhorten the time or produce good leather, but when uſed will be found to add conſiderably to its weight.

5. By means of the hydrometer the comparative value of all ſorts of bark may be eaſily determined. For this purpoſe take equal quantities of the barks to be compared; infuſe them the ſame number of times in equal quantities of water, and the hydrometer will ſhow their relative ſtrength or value.

IV. *On the attractive Power of Bodies floating on Water.*

By Dr. G. CARRADORI*.

IF a few ſmall dry bodies, ſuch as bits of dry leaves or ſaw-duſt, be thrown into water and immerſed at about the diſtance of an inch under each other, during the time of immerſion you will ſee the bodies ſeparate themſelves a little from each other, owing, as may be readily conceived, to the effects of the motion occaſioned by immerſing them in the fluid. If you then bring one of theſe bodies near to another, the latter will every where follow the other, whether you immerſe it deeper, raiſe it above the water, or move it horizontally; and both bodies will hang to each other, and thus exerciſe a kind of attractive force. This, however, is not the effect of the reciprocal attraction of the bodies; for, in the firſt caſe, that is, when you immerſe the one body deeper, the other will endeavour to approach it, becauſe a cavity or circular vacuity is produced, in which it muſt then follow the current; and the cauſe of this is, that the coheſive power of the water is not ſo ſtrong as the attraction which it exerciſes over that body, and therefore its ſurface always yields more, and, keeping behind the body, puſhes it forwards, and, as it were, ſwallows it up. This phænomenon is exhibited by all floating bodies which attract the water; for they follow the ſame movement, which, according to the adheſion which it has with the body, either riſes upwards or ſinks down.

As it has been obſerved that all bodies which contain a certain volatile or fixed oil, or reſin, have an extenſive power, whether they be fluid or hard, and, in the laſt caſe, that they fall into duſt as ſoon as they are applied to the ſurface of the water, I am of opinion that all ſubſtances which exhibit ſuch a phænomenon contain a common oil, or reſin, which gives riſe to it. I will not here mention that ſeries of bodies, all of an oily or reſinous nature, with which I made experiments; but I can aſſert that they all produced this effect in a greater or leſs degree. Milk for example, which, as every one knows, is an expreſſed juice, extends itſelf upon water, as well as the milky juice of the tithymalus and other plants of the like kind, though viſibly with leſs force; and therefore there is no reaſon to doubt that the farina of fruits and other ſeeds is of the ſame nature as an expreſſed juice, as, both in a hard and fluid ſtate, it extends itſelf on the water in the ſame manner as the milky juice of plants. The cauſe of the greater velocity with which farina ſpreads itſelf on water than milk and the milky juice of plants, ought, in my opinion, to be aſcribed, without all doubt, to the reſinous parts which form a difference between it and milk, as the latter contains a fixed oil. This is the caſe not only in regard to common meal, but alſo in regard to the tuberous roots, all the graſſes, and, in general, all plants; and beſides theſe, the farina of a great number of ſeeds of various kinds, both inflammable and incombuſtible, with which I made experiments: mealy fruits have this property in an eminent degree; and on this account we have reaſon to aſſert that all of them have a ſuperabundance of reſinous particles, and that they muſt afford nouriſhing food. There is reaſon to believe that the reſinous part of corn or fruit conſiſts in the gelatine, and therefore I imagined that ſtarch, as a kind of mucilage, muſt be deprived of its property of extending itſelf in water; but it, however, poſſeſſes that property, though in a leſs degree than meal. We cannot therefore refuſe to admit in it a reſinous ſubſtance alſo. It muſt, however, be a ſubſtance more mucilaginous than gelatine itſelf, or reſin; and we muſt aſcribe the difference of theſe two ſubſtances to the proportion of this principle, and perhaps alſo to its different combinations.

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* From *Giornale Fiſico-Medico*, by Brugnatelli, Vol. IV.

Real gum, when divided into as ſmall particles as poſſible, has an extenſive movement on the ſurface of the water alſo. I therefore conſider gum, reſinous gum, and reſin, to be almoſt degrees of one and the ſame ſubſtance.

When I was making experiments of this kind with different ſubſtances, a thought ſtruck me to pound a piece of bread, made of dry grain, and to make experiments with it alſo, in order to aſcertain whether it ſtill retained the property of meal, and I found that it ſtill exhibited the ſame phænomenon. This induced me to conclude that the reſinous part of meal is deſtroyed neither by fermentation nor the proceſs of baking.

Before I quit this ſubject it may not be ſuperfluous to inform thoſe who may wiſh to make experiments of this ſort, particularly with a view to diſcover thoſe ſubſtances which contain oil or reſin, what cautions muſt be uſed to prevent them from falling into errors. Their firſt care, then, ought to be, that the water on which the ſubſtance with which they mean to make their experiments is thrown, may have a large ſurface, and the experiment will be deciſive when it is performed in a veſſel into which the water flows and runs out, or in a river or pond.

If the experiment be made in a veſſel filled with water, oily impurities may eaſily adhere to the ſides of it, and thus prevent the effect, as they will occupy the ſurface of the water.

Care muſt be taken alſo that no oily ſubſtance, of whatever nature, may have been previouſly adhering to the veſſel; for, in that caſe, the experiment would fail.

The evaporation of the body may alſo occaſion deception. I have myſelf obſerved, that when the hand is rubbed under the armpits, over the forehead, or on any other part of the body from which more oily evaporation proceeds than from others, and if a piece of earth be taken in this hand and placed on the water, an extenſive movement immediately takes place. The ſame thing may happen, if the ſubſtance with which the experiment is made be held a long time in the hand. It is therefore neceſſary to waſh the hands, and, after they have been well dried, to proceed to the experiment by throwing the ſubſtance on the water from a ſmall ſpoon. It may thence be readily conceived what errors may ariſe if all theſe precautionary rules are not obſerved.

V. *Hiſtorical Sketch of the Inſtitution and Progreſs of the Royal Society of London.*

BELIEF in the influence of the ſtars on the fortune and character of man, together with the attention always unavoidably given to the meaſurement of time, preſerved ſome knowledge of aſtronomy among our anceſtors even in the darkeſt ages of modern Europe, and rendered it one of the firſt objects of zealous ſtudy at the æra of the general revival of learning and ſcience. The alchemical dream of the convertibility of all baſer metals into gold, engaged many an enthuſiaſt and many an impoſtor in experiments in chemiſtry, from the earlieſt period in modern hiſtory from which we have any information of the purſuits of the inquiſitive and the learned. The uſes of common life, and the reſtoration of this part of the ſcience of the antients, encouraged and advanced the ſtudy of mechanics even in the fifteenth and ſixteenth centuries. No other branch of phyſical ſcience but theſe three had been much or ſucceſsfully ſtudied by the moderns before the days of Bacon.

It was the grand merit of Bacon, that he turned mankind from inveſtigating in ſcience “ merely the relations of exiſtence (in general very imperfectly known), and of words,”—“ to acquaint themſelves more fully, by the experiments of the ſenſes and of auxiliary inſtruments, with exiſtence in its different ſenſible modifications, and with the mutual relations of the various parts of material exiſtence to one another.” Science was thus at once reduced to “ experience, and the arrangement of the facts which experience aſcertained.” The united inquiries, obſervations, and memoranda, of as many as poſſible of the intelligent and enlightened part of the human race, were henceforth underſtood to afford the only means for its true improvement. Men became now firſt ſenſible, that, by due inveſtigation of the qualities and the phænomena of matter, they might diſcover modes of ſcience unknown to the antients. And the neceſſity for the aſſociation of philoſophers, in order to the accompliſhment of the great purpoſes of philoſophical inquiry, was from this time, by thoſe who adopted the views of Bacon, warmly acknowledged.

It dwelt much upon the minds of the ingenious and inquiſitive in England. Milton's plan for a new ſeminary and courſe of education, in his letter to Hartlib, ſeems to have been ſuggeſted by this fundamental idea. Cowley's propoſal of a philoſophical college, to be eſtabliſhed, with an income of four thouſand pounds a year, at Chelſea, was a fine model for the union of a ſchool with a ſociety for the advancement of phyſical knowledge. It was within a few years after the death of Charles the Firſt, that a few of thoſe perſons, who were afterwards incorporated in the Royal Society of London, began at Dr. Wilkins's lodgings, in Wadham college, Oxford, thoſe philoſophical meetings which were to be in that ſociety continued.

Theſe were almoſt the firſt philoſophical meetings in Europe for the advancement of phyſical knowledge. There were in Italy more than forty academies, for the refining of the Italian language and the ſocial ſtudy of the fine arts. But, though the ſocieties of architects, ſculptors, and painters, might make ſome inquiries and obſervations relative to thoſe parts of phyſics in which their reſpective arts were immediately concerned; yet the opprobrium of atheiſm, which was charged againſt the earlier natural philoſophers in that country, had, with other cauſes, hitherto prevented ſocieties, with expreſs and excluſive views to phyſical diſcovery, from there ariſing. In France, an academy had been inſtituted at Paris for the refinement of the French language. A ſimilar eſtabliſhment began to be formed at Caen. At Aix, in Provence, the illuſtrious Peireſc, the Robert Boyle of France, not only correſponded with the moſt eminent and ingenious men in Europe on almoſt every intereſting ſubject in phyſics and in the ſtudy of antiquities, but engaged his friends often to aſſemble in his houſe for the mutual communication of their obſervations and diſcoveries as well in natural philoſophy as in every other part of knowledge. But the inſtitution of an academy of ſciences was reſerved to diſtinguiſh, at a period ſomewhat later, the miniſtry of Colbert, and the moſt ſplendid part of the reign of Lewis the Fourteenth. The profeſſors in the German univerſities occaſionally met and converſed about the ſubjects and ends of their ſtudies; but in theſe, phyſical ſcience had, hitherto, but ſmall ſhare; and there was not as yet, in Germany, any ſeparate company of philoſophers aſſociated for the inveſtigation of the laws of material nature.

Dr. Seth Ward, the Hon. Robert Boyle, Dr. Wilkins, Sir William Petty, Mr. Matthew Wren, Dr. Wallis, Dr. Goddard, Dr. Willis, Dr. Bathurſt, Dr. Chriſtopher Wren, and Mr. Rooke, were the principal perſons who aſſociated at the meetings in Wadham college. They met rather to perform experiments than to diſcourſe about them. Mechanics and chemiſtry were the branches of ſcience to which their firſt experiments chiefly related. In the year 1658 the ſcene of their meetings was transferred from Oxford to Greſham college, in London. Their numbers were here ſoon conſiderably augmented. Their meetings were, for a time, interrupted by the diſturbances which enſued between the uſurpation of the Cromwells and the reſtoration in 1660.

At the reſtoration, two Scotſmen, Sir Robert Moray, of Tibbermuir, and a Mr. Erſkine, who, having followed the king in his exile, had now influence at court, eagerly joined the philoſophical meetings at Greſham college, and perſuaded their ſovereign to take the riſing ſociety under his own ſpecial patronage. A royal charter was ſoon after granted, incorporating theſe philoſophers into a body, to conſiſt of a preſident, a council of one-and-twenty members, and an indeterminate number of fellows. William Lord Viſcount Brounker became their firſt preſident. The inſtitution was, for a time, very much an object of faſhionable attention at court. The king himſelf, as the patron and founder, the duke of Buckingham, prince Rupert, the earl of Clarendon, the firſt earl of Shafteſbury, Monk duke of Albemarle, duke Albert of Brunſwick-Lunenburgh, the earl of Peterborough, were enrolled at the head of a number of diſtinguiſhed names in the firſt liſt of the members—*Omne ignotum pro mirabili.* For a time, the moſt extravagant enthuſiaſm was excited in favour of the new ſociety, and from its exertions an improvement the moſt extraordinary of all the arts of life was expected at once to enſue.

The meetings of the ſociety continued after its incorporation by charter, as before, to be held at Greſham college. They took place every Wedneſday in term time, immediately after the lecture of the profeſſor of aſtronomy in that college, and were eagerly attended by the members. Dr. Croone, with the title of regiſter, had, at firſt, the care of recording the tranſactions at theſe meetings in journals. It was afterwards committed to Dr. Wilkins and Mr. Oldenburgh, the two firſt ſecretaries. Cowley had demanded a revenue of four thouſand pounds a year for his projected college at Chelſea; but the Royal Society was ſupported without any public fund, ſimply by the voluntary contributions of the gentlemen of whom it was compoſed.

The ſociety entered upon its reſearches with views at once magnificent and philoſophically modeſt. It propoſed but "to make faithful records of all the works of nature and art." By theſe it hoped to explode errors, to reſtore neglected truths, to apply philoſophy to the uſes of life, and to open up the avenues to future diſcovery. It profeſſed to admit into its aſſociation men of all religions, all countries, all conditions and employments in life. They propoſed to derive uſeful truth not only from the learned and profeſſed philoſophers, but from the ſhops of mechanics, the voyages of merchants, the ploughs of huſbandmen, the ſports, fiſhponds, parks, and gardens, of gentlemen Some heſitated whether Mr. Grant, a ſmall ſhopkeeper in London, the author of ſome excellent obſervations on the bills of mortality, ſhould be received into ſuch a company of philoſophers. But king Charles, on peruſing his obſervations, not only recommended his immediate admiſſion, but deſired that they would add to their number as many ſhopkeepers as poſſible of equal endowments.

In their reſearches they would not truſt the reports of others in any caſe where they could bring the matter under the examination of their own ſenſes. In ſome inſtances they allowed particular members to chooſe whatever ſubjects they ſhould think proper; to inſtitute, at pleaſure, any train of experiments on them; to defray the expenſe from the purſe of the ſociety; and then to report at the meetings the ſeries and the reſults of the experiments they had thus made.

Other experiments were expreſsly ordered by the voice of the whole ſociety, and were intruſted in the performance, not to ſingle perſons, but to committees nominated for the occaſion. For inquiry concerning things at a diſtance, they had, very early, begun a correſpondence with the inquiſitive and enlightened in almoſt every different part of the world. At many experiments in the parks, in the royal gardens, and on the Thames, the king himſelf zealouſly aſſiſted in perſon. Mr. Huygens, the famous philoſopher in Flanders, received, ſoon after the inſtitution of this ſociety, its frequent aſſiſtance, and communicated to it all his diſcoveries and inventions; in particular, that noble one of the firſt application of the motion of pendulums to clocks and watches. The philoſophers of Florence, and eſpecially the grand duke's brother, prince Leopold, entered into friendly correſpondence with the Engliſh philoſophers, and acquainted them with every new obſervation that was made on nature in Italy. The Germans no ſooner heard of this inſtitution than they ſent many of their books to be ſubmitted to its cenſure, and contributed likewiſe preſents of various new inſtruments of their invention. The travellers, phyſicians, ſurgeons, anatomiſts, &c. of France were peculiarly early and frank in their communications to it. The Academy of Arts and Sciences at Paris was ſoon after embodied in imitation of this ſociety: and the example of the Engliſh was quickly, more or leſs, followed in almoſt every country of Europe in favour of the phyſical ſciences and the uſeful arts.

The experiments which king Charles the ſecond was the moſt attentive to, related chiefly to chemiſtry and mechanics. He would often amuſe himſelf at Whitehall in the execution of curious mechanical works. He had alſo in his palace a ſmall laboratory for chemical operations. He made aſtronomical obſervations, upon various occaſions, in Saint James's Park. He was exceedingly curious in directing the inquiries of the ſociety to every thing that concerned the improvement of the art of ſhip-building. He attended and directed different experiments at the Tower of London, as well as at Whitehall, to aſcertain the forces of projectiles, with their variations in different circumſtances.

In their early meetings, the members of the ſociety were at great pains to diſtinguiſh what objects were the worthieſt of their firſt inquiries. Some were directed to examine all "treatiſes and deſcriptions" of the natural and artificial productions of foreign countries: others were commiſſioned to diſcourſe with ſeamen, travellers, tradeſmen, and merchants, by whom thoſe countries and their productions had been perſonally examined. From the union of the *vivâ voce* with the written intelligence papers were formed, explaining ſummarily what was known concerning every different country, and what remained to be yet inquired into. Theſe papers were read at ſubſequent meetings; and queries were then drawn from them, to be tranſmitted for anſwers to the ſociety's foreign correſpondents.

One ſet of queries which were prepared among the earlier labours of this ſociety, had for its object, to mark "what things were needful to be obſerved in order to the making of a natural hiſtory in general." They formed ſimilar queries reſpecting the hiſtories of the air, atmoſphere, and weather; of the production, growth, &c. of vegetables; of agriculture, &c. They marked out trains of experiments to be tried on rarefaction and condenſation; on the petrifaction of wood; on the loadſtone; on the diſcoveries wanted to complete the ſcience of anatomy; on injections into the blood of animals, and on the transfuſion of the blood of one animal into another: on the tides of the ſea; on the varieties of oyſters, and the manner of their nouriſhment; and on the phænomena of mines. They gave directions to ſeamen for the obſervation of the eclipſes of the moon, the eclipſes of the ſun by Mercury, and the ſatellites of Jupiter. They ſent abroad directions for obſervations and experiments to be made in India, China, the iſland of St. Helena, Teneriffe, Guinea, Barbary, Morocco, Spain, Portugal, Turkey, France, Italy, Germany, Hungary, Tranſylvania, Poland, Sweden, Iceland, and Greenland.

One of the firſt anſwers they received to their inquiries concerning foreign countries, was ſent from Batavia by Sir Philiberto Vernatti. It corrected an error, prevalent in Europe, "that diamonds and other precious ſtones were in the Indian iſles continually renewed, as by vegetation, in the quarries out of which they were taken." It informed them of the exiſtence of a volcanic mountain in Java, and another in Sumatra. It exploded a vulgar error, which one ſhould have thought to be, even otherwiſe, incapable of obtaining for a moment the aſſent of philoſophers—"that there was in the iſland of Sambrero, lying north from Sumatra, a ſenſitive plant that would, when pulled with the hand, ſhrink into the earth, and that had for its root a living worm." It brought one of the firſt accounts propagated in Europe of the exiſtence of the *bohan upas*, the famous poiſon-tree of Java. It added a variety of other curious information concerning the moſt extraordinary objects and arts in the Eaſt. But of the queries ſent out, it left ſtill many unanſwered.

Mr. Hooke was one of the moſt ingenious and indefatigable of the early members of this ſociety. He explained, in an admirable paper, the beſt method of making a hiſtory of the phænomena of the weather. He made many experiments, which were repeated, with many variations of circumſtances, by the ſociety, to prove, "that viſible and ſenſible fire was but the act of the diſſolution of heated bodies by the air: that this diſſolution of ſuch bodies took place in a manner perfectly ſimilar to the ſolution of iron, tin, and copper, by acids: that the evolution of heat and light was a neceſſary reſult of the ſolution of heated and combuſtible bodies in air: that the aſhes of a burnt body were a reſiduum of it, that was inſoluble in air." This chemical theory, as the learned reader cannot but perceive, differs from that which was propoſed by M. Lavoiſier, and is now deſervedly received as philoſophical truth, only in being expreſſed in the chemical dialect of that time, which is now obſolete; and in being, in the intention of its author, to a certain degree accommodated to other theories which then prevailed. In verifying the theory of Hooke, the ſociety made a number of experiments: to aſcertain how long a candle, a lamp, or ignited coals, would continue to burn in a cubic foot of common, of rarefied, and of condenſed air; to exhibit the ſudden extinction of flame by the affuſion of air already ſatiated with burning, and the impoſſibility of even the moſt intenſe and torrid heat to continue without a ſupply of freſh air; and to find what particular degree of heat was to be produced from the burning of every different combuſtible material. By other experiments, having a reſpect to the ſame theory, they aſcertained that flame was ſubject to be extinguiſhed by the air in a deep well. They made beſide theſe, and with a view to diſcern the varied phænomena of ignition and combuſtion, independently of Mr. Hooke's ſyſtem, a great number of other experiments on the bringing of copper into a ſtate of great combuſtibility, on the igniting of tin filings by the help of nitre, &c. &c.

They inſtituted a multitude of experiments to determine the nature, properties, and uſes of air. By the care eſpecially of Mr. Boyle, many trials were made to find, by means of the baroſcope, the different degrees of the preſſure or gravity of the atmoſphere at different heights and depths. They took much pains to diſcover the oppoſite limits of the rarefaction and the condenſation of air. They made a variety of experiments on the propagation of ſound in air: and what is, in a chemical point of view, highly remarkable, a number of their experiments were directed to aſcertain "the generation of air, by corroſive menſtrua, out of fermenting liquors; and to determine the fitneſs or unfitneſs of ſuch air to ſupport combuſtion and reſpiration." Other experiments were made by them for the purpoſe of diſcovering how long a man might live by inſpiring and reſpiring the ſame air; whether air contaminated by reſpiration might be again made pure and reſpirable; whether the unfitneſs of contaminated air for reſpiration were not a quality altogether independent of its temperature, as hot or cold. By other experiments they examined what quantity of air might be ſufficient for the reſpiration of an animal for a given ſpace of time; and in what manner air, previouſly applied to ſupport combuſtion, became by that unfit to be reſpired. Others of theſe experiments reſpected the neceſſity of air to ſupport vegetation; and it was the object of others to examine what uſe was made of air in the vital functions of fiſhes under water.

Water was the immediate ſubject of many of their firſt experiments. Some of theſe were made to aſcertain the differences in the ſpecific gravities of different ſorts of water. The Torricellian experiment on the aſcent of water *in vacuo* was repeated in almoſt every poſſible change of circumſtances. Other experiments were made on the production and phænomena of ſteam. By others they endeavoured to find the differences in the heat of the waters of the ocean at different depths under the ſurface.

On ſtones and metals they inſtituted many experiments. Lead, diamonds, the Bologna ſtone, the ſeparation of ſilver from lead, and eſpecially the loadſtone and the magnetized needle, were now firſt philoſophically examined in regard to ſome of their moſt important qualities.

They examined the growth of vegetables in different ſorts of water; the utility of ſteeping ſeeds; the inverſion of the roots of plants ſet for growth; the decreaſe of the weight of plants growing in air; the reunion of the bark to the wood from which it had been ſtripped.

Their medical and anatomical experiments were numerous. They diſſected a cameleon: they made injections into the veins of different animals: they made a number of obſervations and experiments with a view to determine how far there might be truth or falſity in the doctrine of the equivocal generation of inſects: they made many trials on the meaner animals of poiſons and antidotes: they tried what effects might enſue from the transfuſion of the blood of one animal into the veins of another: and they made likewiſe ſome curious experiments on the hatching of ſilk-worms in rarefied air.

Their experiments on the freezing of water in different circumſtances; on the production of cold by ſaline ſolutions; on ice, to evince that it was ſuſceptible of various degrees of cold more intenſe than that of ſimple freezing; and on the congelation of oils; were various, intereſting, and proſecuted with the moſt attentive accuracy. A curious train of experiments was made at the Tower, under the immediate inſpection of the lord viſcount Brounker, to aſcertain what changes might be produced on the weights of lead and copper by ſubjection to fire in a cupel. Both the copper and the lead were found to gain (by oxidation, no doubt,) an addition. The cupel ſuffered always a diminution of its weight when ignited, but not a diminution equal to the augmentation in the weight of the metals.

Among the inſtruments invented by the ſociety within a few years after its inſtitution were, an univerſal ſtandard meaſure of magnitudes by means of the pendulum; a wheel barometer, and other inſtruments, for finding the preſſure of the air; an auger for boring the ground, and fetching up parts of the ſtrata through which it ſhould paſs in their natural order; an inſtrument for meaſuring the ſwiftneſs and ſtrength of the wind; a diving-bell, and a pair of ſpectacles with which a diver might ſee any thing diſtinctly under water; ſeveral engines for finding and determining the force of gunpowder; ſeveral acouſtic inſtruments to aſſiſt and improve the ſenſe of hearing; a *chariot way-wiſer*, which would exactly meaſure the length of the way of any coach or chariot to which it was applied; an inſtrument for grinding optical glaſſes; a variety of teleſcopes, &c. &c.

A manufacture of glaſs had been, more than thirty years before, eſtabliſhed in Broad-ſtreet, in London, by a company of mercantile adventurers, among whom the moſt conſiderable perſon was admiral Sir Robert Manſel. Workmen and ſuperintendents were procured from Venice. It might have gone on ſucceſsfully, if the great civil war had not broken out. Soon after the reſtoration, the duke of Buckingham, at a vaſt expenſe, eſtabliſhed new glaſs-works in London; and the art was there, within a very few years, carried to ſuch perfection, that theſe works ſupplied better glaſs for microſcopes and teleſcopes than that which was to be had from Venice. The duke of Buckingham not only expended much money upon thoſe glaſs-works, rather as an experimentaliſt than a manufacturer, but took a warm and active intereſt in various others of the ſociety's purſuits; and it was under his immediate patronage that Dr. Spratt wrote his excellent Hiſtory of the Royal Society—the fineſt piece of Engliſh proſe that was produced in the ſeventeenth century.

[To be continued.]

VI. *Chariot Way-wiſer—Pedometer—known to the Antients.*

By Mr. R. HERON.

To Mr. Tilloch.

SIR,

I PERCEIVE, by ſome excellent articles which have at different times appeared in your Magazine, that you are not averſe to illuſtrate the ſciences and arts by the lights of claſſical erudition. I cannot pretend that there is much erudition in having particularly remarked the following paſſage in the life of the Roman emperor Pertinax by Julius Capitolinus. But it certainly evinces at leaſt one ingenious mode of mechanical art, which I had ſuppoſed peculiar to the moderns, to have, on the contrary, had a diſtinct exiſtence likewiſe among the antients.

This paſſage occurs where the biographer has occaſion to relate, that Pertinax, to find money for a donative to the ſoldiers of the prætorian guard, ſold off, by a nine days auction, the ſumptuous dreſſes, furniture, and curioſities of art, with which Commodus had filled the palace. It is a part of an abridged inventory of the things ſold at that auction. It conſiſts of two clauſes: I ſpeak, for the preſent, chiefly of the latter.

"1. Vehicula, arte fabricata nova, perplexis diverſiſque rotarum orbibus, et exquiſitis ſedibus,—nunc ad ſolem declinandum, nunc ad ſpiritus opportunitatem, per vertiginem: 2. Et alia, iter metientia, et horas monſtrantia."

"1. Carriages of a new conſtruction, with wheels of different diameters, and curiouſly connected with one another, and with ſeats of a peculiar and exquiſite contrivance; in which vehicles the rider could at pleaſure ſhade himſelf from the ſun, and was in no danger of getting out of breath; ſo eaſy was their motion at its utmoſt ſwiftneſs. 2. Alſo other carriages, which had contrivances to meaſure the diſtances over which they were driven, and to count the hours ſpent in the journey."

Here are two ſorts of carriages mentioned, both of new conſtruction. The former, remarkable for the eaſe of their motion, in conſequence of having wheels of different diameters, and for covering the rider from the ſun, may ſeem to reſemble ſome of our preſent carriages more nearly than has been ſuppoſed, yet do not ſo ſtrikingly differ from thoſe which were common among the antients as to excite much ſurpriſe. But the latter appear, indeed, extraordinary: they poſſeſſed all the advantages, as it ſhould ſeem, of the *chariot way-wiſer*, which has been believed to have been firſt invented by ſome members of our Royal Society about the year 1662; and of the *pedometer*, which is at preſent manufactured, I think, under a patent.

I am, Sir,
Your obedient ſervant,
R. HERON.

London,
October 8, 1801.

VII. *Obſervations on the Spots of the Sun, and the Nature of the Light of that Luminary. By* M. VON HAHN *.

OUR experience of the various effects produced by the ſun's rays, and particularly the doctrine of latent heat and ſpecific heat, ſeem to confirm the opinion that light is an actual body which ſtreams forth from the ſun, and which is connected, in a variety of ways, with the bodies of our earth. The determination of the following queſtion, which belongs peculiarly to chemiſtry, "Whether the ſun be actually a burning body, or only attracts the illuminating matter from the ſurrounding expanſe of the univerſe?" may receive ſome illuſtration from continued obſervations made on the body of that luminary.

One great difficulty here is, that the ſun has a peculiar light, and we are accuſtomed only to conſider objects which almoſt continually receive their light from without, and which commonly produce ſhadows more or leſs perceptible according to the different directions and ſtrength of the light which falls upon them. Now our eyes, being accuſtomed to ſhadows and projecting images, by beholding an uncommon kind of illumination are thrown into embarraſſment, and cannot bring the images in harmony with ſenſation, the baſis of all perſpicuity.

One may obſerve the ſun a long time without diſcovering any other ſpots than thoſe which are common. The firſt view, therefore, of the ſo called ſolar faculæ produces an agreeable ſurpriſe, as it exhibits ſomething very different from what we ſee in thoſe parts of ſurrounding nature which are neareſt to us. With a Herſchel's teleſcope of ſeven feet, one beholds with aſtoniſhment, and very diſtinctly, in thoſe ſpots in the middle of the ſun's orb, parts beſet with little hills ſeparated by valleys, and ſingle mountains, of a proportional height, which project ſtrong ſhadows. It is worthy of remark, that theſe objects have as diminiſhed an appearance as when one looks at things from a great height. The cauſe of this is to be ſought for in the ſituation of the light which falls on them, and belongs to thoſe optical deceptions that muſt occur ſo frequently in obſerving the ſun, and on a knowledge of them depends the whole ſecret of forming a proper judgment reſpecting the ſolar bodies.

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* From *Profeſſor Bode's Aſtronomical Almanac for the Year* 1796.

The fineneſs of the delineation, the ſcenic appearance, and the regularity of theſe objects, induce one to believe that ſolar landſcapes are here actually repreſented; and totally deſtroy the idea of the ſun being a flaming body; for it is impoſſible that the irregular movement of flame could ever produce ſuch ſcenes. Theſe parts are, at times, of a more whitiſh colour; but it ought to be particularly remarked that they have a ſomewhat dimmer light than the reſt of the ſun's orbit, which is every where luminous. If the diviſion of the lights and ſhadows, the kind of illumination, and the gradation of the ſhades, be accurately obſerved and examined, we ſhall be obliged to admit that theſe faculæ in the ſun are landſcapes.

The moſt important obſervation here is, that when there are a group of ſuch landſcapes the ſhadows are almoſt always projected to one ſide, and are ſharp-pointed. This circumſtance ſerves to render clear to the eye a confuſed mixture of ſpots.

Let one ſearch out, in the neighbourhood of ſuch groups, ſmall, ſingle, inſulated ſpots, and obſerve whether there is ſeen near them a weak gradation of ſhade, or a luminous vapour which is very familiar to us, and which ſhows that the light illuminates an eminence on all ſides, but not with equal ſtrength. If the eye is once accuſtomed to the effect that hence ariſes, it can with great eaſe convert an often exceedingly compound mixture of dark parts and luminous ſpots into well arranged images.

There are certainly caſes in obſerving the ſun when draftſmen and painters only could be able to arrange properly the mixture of faint ſpots, and to obviate the deception which ariſes from half ſhadows. When an eminence, for example, is ſo ſtrongly enlightened by the luminous matter near it, that one ſide of it has the ſame brightneſs as the ſolar light itſelf, we then no longer ſee an eminence, but only a faint ſpot produced by the half ſhadow on the other ſide. But if the direction of the ſtrongeſt half ſhadows towards any part of the ſun's diſk be once known, we may then venture to examine the greateſt collection of luminous faculæ, and diſtinguiſh them with the beſt ſucceſs. They then loſe their ſtrange appearance; and the eye becomes convinced that an aſſemblage of eminences and plains could exhibit ſuch an appearance only by a light proceeding from different ſides, though with various degrees of ſtrength. Beſides, it may ſometimes be the caſe that the light may be higher, nearer, or at a greater diſtance in one diſtrict than another; and therefore it is difficult, under all theſe circumſtances, to form a right judgment of an object in the ſun.

The ſun, therefore, muſt be incloſed in a luminous covering, or ſurrounded by an atmoſphere of light—an idea firſt ſtarted by Mr. Bode; but the peculiar quality of it remains to be more fully inveſtigated. A globular covering of boiling light ſeems to be too moveable to produce the above phænomena. The irregular ſpreading of the ſurrounding flame would every moment change the ſcene; as a mixture of luminous matter with the ſolar atmoſphere would give it an obſcure appearance, and be contrary to the ſmoothneſs of that line by which the ſolar diſk is bounded.

The ſun may be conſidered as an opake body of itſelf, and ſurrounded by an atmoſphere almoſt ſimilar to ours; but the luminous matter as an uncommonly fine fluid, which, like liquid naphtha, floats on this atmoſphere, and, on account of its great lightneſs, can never ſink down under any circumſtances whatever.

The atmoſphere of the ſun, by his rotation, being raiſed higher under the equator, will be depreſſed towards the polar regions. The luminous matter is attracted from the celeſtial expanſe; and this hypotheſis, which has been adopted by many, appears to be the true one, becauſe the phænomena on the ſurface of the ſun can be beſt explained by it. It muſt however be premiſed, that no more luminous matter is attracted than may be neceſſary to form almoſt a total covering to the ſun; and that, on account of the continual influx towards the poles, it muſt often happen that the quantity there will be diminiſhed. This is a conſequence of the thing itſelf; for, if it be admitted that the ſeparation of the luminous matter is performed by a chemical affinity, the effect of which is facilitated by the ſun's rotation, the attraction will ceaſe when the ſolar atmoſphere has imbibed a ſufficient quantity. By obſerving the ſun, we perceive not only the luminous matter, but the light reflected, in a lively manner, from the ſun's body; and at ſuch a diſtance they cannot be diſtinguiſhed from each other. Through a deception of our ſenſes, we often take the effect for the cauſe, and ſeek for the object in the light, and then again for the light in the object.

But as the atmoſphere, greatly expanded at the equator, muſt ſink down ſomewhere from well known cauſes, the luminous matter flies off on all ſides towards the lower parts, and illuminates the parts it left, no longer from above, but laterally. Here we diſcover the ſolar faculæ; and if in thoſe parts there be inequalities, there ariſe then half ſhadows, which are almoſt loſt in the tint of the ground. If the obſerver fails in his judgment of the angle of inflection, he ſees only a mixture of faint ſpots, and no landſcapes.

It will now be eaſier to explain the proper ſpots in the ſun, as they are called. They are parts of the ſun lying under full ſhadows, which muſt naturally ariſe when the illuminating fluid draws itſelf ſo far back from the horizon of that neighbourhood, that no more rays can reach its centre. The contraſt with the reſt of the dazzling diſk of the ſun increaſes their blackneſs, and gives them the appearance of a burnt coal, or cinder. We ſhall be readily convinced that ſome parts of the ſun's ſurface may lie in a full ſhadow, when we reflect on the great ſize of theſe ſpots, and conſider the height of the luminous fluid to be ſmall, or equal to about the diſtance of our higheſt clouds from the earth. It cannot be higher, elſe no perceptible half ſhadows would be occaſioned, becauſe the ſhadows of bodies become ſmaller in proportion to the height of the illuminating point. The irregularity of the ſpots ariſes partly from the different ſituations of the luminous matter, and partly alſo from the inequality of the bottom. That remarkable vapour by which they are ſurrounded is occaſioned by a kind of crepuſculum, by which the ſide light is conveyed to the moſt diſtant part from the centre of the ſhadow. As the luminous matter now appears to be more ſparingly divided in the ſolar atmoſphere, a place may be totally deprived of light by its retiring: but near the boundary of the light it is directed towards the bottom, though ſomewhat weakened by the refraction and reflection of the rays, as is the caſe in our morning and evening crepuſcula; and the ſpots by theſe means obtain their conical form. If we conſider this vapour more attentively, the before-mentioned landſcapes can be clearly diſtinguiſhed according to the above method; and if the full ſhadows have conſiderable inequalities, there ariſe forms of all kinds which exhibit no determined figure, until we become no more acquainted with the ſituation of the ſhadows. On the ſummits of ſuch eminences may be clearly ſeen rows of luminous points, which, however, ariſe from a reflected light; but, on account of their livelineſs, are taken for the luminous matter itſelf. They ſeem to be, as it were, ſeparated portions of the ſolar atmoſphere, which float about within the conical cavities. But on cloſer examination this deception vaniſhes.

Laſtly, ſhould we ſuppoſe an atmoſphere filled with clouds, enlightened from above by the luminous matter, the great blackneſs of the ſpots, and the matter which ſurrounds them, would oppoſe ſuch an idea. Such a continual phænomenon is founded on invariable cauſes, and cannot be reconciled with the changeable nature of clouds. A real ſolar landſcape may be recognized to be the ſame before ſeen, even after ſome time, when the direction of the light which falls upon it has changed.

It is more than conjecture that the ſun ſeparates the luminous matter from the firmament, and partly appropriates it to itſelf and partly tranſmits it to other bodies, which, on account of their leſs magnitude, are unable to effect the ſame. As ſo many ſtars ſhine with their own light, it is probable that they all derive it from the ſame ſource; and that the immenſe ſpace of the firmament contains this all-enlivening matter, which nature has dealt out with a ſparing hand, and ſeems to have deſtined for the moſt important purpoſes.

The phænomenon of the zodiacal light may be very naturally explained according to theſe ideas. The ſeparation of the light takes place more at the ſun; it floats on its atmoſphere in exceedingly ſmall quantity; and, as it ſtreams out immediately in all directions, the terreſtrial bodies are ſecured from immoderate heat by this tendency to diverge. The attraction of the luminous matter from the firmament muſt extend to a conſiderable diſtance; for that ſet free at the ſun is ſupplied by another quantity, which ſeeks for an equilibrium, and therefore forces itſelf towards the ſolar equator. It accumulates therefore in thoſe parts; and, as it is half ſeparated, on account of the attraction of the ſun acting at that diſtance, this mean ſtate produces the paleneſs of the zodiacal light. This conjecture ſeems to be confirmed by a remark of Caſſini. At a time when the ſolar ſpots were inviſible, no zodiacal light was to be ſeen. The abſence of the ſpots ſhows a ſuperabundance of the luminous matter on the ſun; conſequently none is ſeparated, and theſe phænomena muſt ceaſe. Were their ſeat in the ſolar atmoſphere, they ſhow themſelves more ſtrongly the more widely the light is diſperſed over the body of the ſun.

Poſterity, perhaps, will be able to deduce the cauſe of the northern lights and other phænomena from theſe ſuggeſtions reſpecting the production of the luminous matter, and to anſwer the important queſtion—" By what circulation is that noble matter, which diffuſes light and heat throughout all nature, again conveyed back to infinite ſpace; or whether deſtined and employed for the formation and maintenance of organic bodies?"

VIII. *Experiments on Platina.* *By* L. PROUST [*].

I. *Of the foreign Bodies mixed with native or crude Platina.*

THE bodies generally mixed with platina are ſtony and metallic ſand, pyrites, gold, and mercury. The firſt kind of ſand conſiſts of ſmall rock cryſtals and fragments of a hyacinth colour; the ſecond conſiſts of ferruginous ſand of two ſorts: the one octaedral, and ſuſceptible of being attracted by the magnet, among which there are ſome that do not poſſeſs that property till after being heated: the other ſort is equally black and angular; but the angles are deſtroyed in ſuch a manner, that it is not poſſible to determine their figure, which ſeems to announce that they are not ſo hard as the preceding. The latter are not ſuſceptible of being attracted by the magnet, even after they are heated, and when analyſed they exhibit new ſubſtances combined with iron; phænomena by which we are authorized to conſider them in their union as a mineralization of a peculiar kind, as will be ſeen hereafter.

II. *On the Method of ſeparating the Sand from the native or crude Platina.*

Place three or four pounds of crude platina on a large table the edges of which are ſomewhat raiſed, and at the diſtance of a foot from one of the ſides; then ſpread it out, by means of a card or a rule, in ſuch a manner as to occupy the ſpace of about two feet, and blow upon it obliquely with a pair of hand bellows, to make the ſand, which is always lighter than the gold or platina, paſs to the oppoſite ſide. If this operation be repeated five or ſix times, taking care to blow in different directions, the platina may be rendered ſo pure that, when examined with a magnifying glaſs, a grain of ſand will not be found in a pound of it.

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[*] From *Annales de Hiſtoria Natural,* Madrid 1799, No. 1.

III. *Of the Gold.*

All the gold contained in crude platina is ſeldom diſcovered, even with the help of a magnifying glaſs; and as mercury is the means employed to ſeparate them, it is evident that, if a few grains of gold remain, they muſt retain their portion of the mercury. Hence the gold acquires a white hue, which aſſimilates it to the colour of the platina, and prevents it from being perceived.

I do not know whether the workmen at the platina mines are acquainted with any certain method of aſcertaining whether the ſeparation be complete. A very ſimple proceſs is, to heat the platina in an iron ſpoon or crucible. The mercury becomes volatilized, the gold reſumes its colour, and the platina acquires that of calcined iron. The gold then, on account of its colour, may be very eaſily diſtinguiſhed from the platina.

In this manner I ſeparated 7 ounces of gold grains, or ſcales, from 100 ounces of ſome kinds of crude platina, and 13 from others. The mean product is 10 per cent., as I announced at the time to government.

Theſe ſcales are of different colours: ſome have the yellow colour of gold, others are of a pale or a green yellow. The laſt, tried with the blowpipe and borax, fine themſelves, tingeing the borax green, as iron would do; which ſhows that it is a green native gold, like that of the jewellers.

The nitric acid alſo, digeſted cold over theſe kinds of crude platina, ſeparates the mercury from the gold, but not ſo completely as heat.

Crude platina of a black colour, finer than the white, is very commonly brought from America. It ſtains the fingers, probably on account of the ſuperabundance of graphite or plombago which it contains. I never found gold in this kind, and I have not been able to obtain any information reſpecting its origin.

IV. *Of Sulphur as the Mineralizer of Platina.*

When crude platina is expoſed to the heat of the blowpipe on charcoal, a ſtrong ſmell of ſulphur is exhaled, accompanied with fumes which do not whiten gold, and which are not produced but by a degree of heat ſuperior to that which volatilizes mercury. Black platina grains give more fumes than the white.

Large grains of platina picked and heated, emit alſo an odour of ſulphur accompanied with fumes. A perſon employed by government to collect platina informed me, that the largeſt grains he ever ſaw never exceeded the ſize of a pea; and that he had given ſeveral of that ſize to the archbiſhop of Santa-Fe. In regard to the enormous grain ſaid to be depoſited in the cabinet of Vergara, the director of the Biſcayan ſociety aſſured me that he had no knowledge of it.

When crude platina is brought to a red heat in a covered crucible, no odour of ſulphur is perceived with the vapour of the charcoal, but at the moment when the crucible is uncovered this odour manifeſts itſelf very powerfully; but if the crucible be ſhut cloſe it ceaſes. It was long before I was able to diſcover the nature of this vapour; but the experiments, which will be deſcribed hereafter, proved to me that it is nothing elſe than concentrated ſulphuric acid.

From 1600 grains of white crude platina well purified, and heated in a retort, I obtained only half a grain of mercury and a grain and a half of water, without any kind of gas. Having broken the retort before it was quite cold, it exhaled a ſulphurous odour. The platina became black, and was found ſlightly cemented, and adhering to the glaſs.

A mixture of 400 grains of the ſame crude platina and an equal quantity of ſweet muriate of mercury, heated in a retort, gave, after the ſublimation of the mercury, a very light ſtratum of cinnabar, which adhered to the top of the retort, and, when ſcraped a little, aſſumed a vermilion colour.

Crude platina, heated in a crucible and immediately poured into a ſilver diſh, blackens the veſſel in the ſame manner as ſulphur. In a word, all ſolutions of platina form an abundant precipitate with barytes.

As ſulphurets of platina, both native and artificial do not ſuffer their ſulphur to eſcape when heated in covered veſſels, there is reaſon to believe that this metal cannot be entirely freed from it by an open fire; for the ſulphuret which occupies the centre of each grain, may then be conſidered as ſhut up in a veſſel.

Here, then, we have ſulphur as a conſtituent part of crude platina. It may here be aſked, with what combuſtible this ſulphur is united. If it be recollected that iron, and even copper, exiſts alſo in this ore, it will not be ſuppoſed that it is combined with the platina; and yet it is with this metal that the ſulphur is united, as will be ſeen hereafter. In the mean time it may be deduced, from the preceding experiments, that what is at preſent called, and what ought to be continued to be called, *crude platina*, is evidently nothing elſe than the mineralization or ſulphuret of that metal which it has been thought proper, in the modern nomenclature of metallic ſubſtances, to call *platina*.

V. *On the Oxidation of crude Platina.*

When native platina is brought to a red heat in open veſſels, it aſſumes the appearance of the oxidated iron of a gun barrel; but with this difference, that the oxidation effected at the ſurface is found at its *maximum*; for by rubbing it on paper, or triturating it with water in a porcelain mortar, a red powder is detached from it. Another proof of this is obtained by digeſting over it muriatic acid, which entirely diſſolves this powder, and with alkalies gives a precipitate of oxidated iron of 48 per cent. The platina which remains has recovered its whiteneſs.

I ſhall here give an account of an experiment made a good many years ago by C. Darcet, and at which I was preſent. That chemiſt put into a crucible, placed in the porcelain furnace of Serres, ſome ounces of crude platina: when taken from the furnace the platina was triturated with water, and ſeparated by theſe means from all the powder it contained. After this firſt ſubſtance was removed from it, it was put again into the furnace, and purified a ſecond time in the ſame manner; and this operation, being repeated, gave a new quantity of powder. After paſſing through the furnace in this manner, as far as I can remember, ſeven or eight times, the platina was totally changed into an oxide of a very dark brown colour. I was prevented, by particular circumſtances, from learning of Darcet what was the reſult of his laſt experiments.

An account of experiments made on platina in the porcelain furnace of Furſtemburg may be ſeen in the *Journal de Phyſique*; but as the author did not take care to remove the oxide as it was formed, he concluded, as Macquer, Baumé, and other chemiſts had done that platina was indeſtructible. But let us return to the oxidation in queſtion.

The muriatic acid, of which we have ſpoken, had taken from the calcined platina three oxides, viz. that of iron, that of copper, and that of platina. This ſolution, united with the ſolution of muriate of ammonia, gave the yellow precipitate which is peculiar to platina.

Potaſh being employed to complete the precipitation from the liquor, the reſult was a ſecond precipitate of red oxide of iron, mixed with a little copper, which immediately manifeſted itſelf by muriate of ammonia.

If in this ſolution there had been found only a little oxide, which the muriatic acid might have ſur-oxygenated, the ſolution lation of platina would have exhibited nothing aſtoniſhing. But as its deoxidation could not be aſcribed to ſuch a cauſe, it muſt be inferred, that when platina is combined with other metals, it becomes oxidated much eaſier than has hitherto been ſuppoſed. Platina, then, has this property, like all other metals, in which the ſtate of combination always favours oxidation.

The following is the detail of an experiment made with great care. Eight hundred grains of crude white platina, well purified from gold, ſand, and mercury, after having been calcined for the ſpace of an hour, had loſt only one grain. Having afterwards expoſed them to the action of muriatic acid, they loſt in the acid four grains and a half, and reſumed their whiteneſs; by which it is ſeen that the oxygen of the atmoſphere had almoſt replaced the weight of the ſulphur diſſipated.

The $794\frac{1}{2}$ grains, being calcined a ſecond time, became black, and their weight was increaſed two grains, but the ſmell of ſulphur was no longer diſtinguiſhed; a circumſtance which renders the combination of oxygen more ſtriking. Having been again expoſed to the muriatic acid, they loſt $9\frac{1}{2}$ of their weight: they were found to be black, and viſibly corroded.

The 788 grains which remained were boiled a third time in the ſame acid, and the reſult was a new loſs; but that which they experienced in freſh acid, after a fourth ebullition, was ſlow, and far inferior in the two laſt mentioned boilings: it did not exceed four grains and a half.

The weakened action of the acid made me believe that I ought to continue the oxidation in order to penetrate further into the interior of the platina, which induced me to proceed to a fourth calcination; but it was uſeleſs, for the grains experienced no change either in their weight or their colour: the application, therefore, of the muriatic acid was to no purpoſe. The exterior of the grains had then the appearance of pure platina, having a very ſtrong reſemblance to ſilver of a low alloy.

The total loſs ſuſtained by theſe 800 grains was $17\frac{1}{2}$ grains, or a little more than two hundredths. A grain of gold, not perceived in purifying the crude platina, was found towards the end of the operations; and this grain proved to me that the muriatic acid alone produced the effects I have deſcribed, and that nitric acid had no ſhare in them.

The metallic ſolutions of which we have ſpoken contained each platina, iron, and copper: they became turbid with the muriate of ammonia; from which we may infer, that if the platina be diſſolved in the muriatic acid, that it may be precipitated by that ſalt.

Let us now return to the crude platina. When treated with dulcified muriate of mercury, it is found at the bottom of the retort with all the ſigns of corroſion. The water immediately takes from it a light efflorеſcence of a yellowiſh white colour; and potaſh, poured into the liquor, ſeparates from it a red oxide of iron and of copper. In regard to platina, none is found in it; from which we may conclude that the iron and copper only had received from the muriate of mercury a ſufficient quantity of oxygen to effect their ſolution. The loſs of the ore was 12 per cent. We are therefore obliged to acknowledge three metals in platina ore. We ſhall ſhow hereafter, that beſides ſulphur it contains alſo two other combuſtibles *.

VI. *Experiments with the Nitric Acid on Platina.*

Nitric acid of 35 degrees, in which crude platina, purified from ſand, iron, and copper, was boiled, did not even alter the poliſh of the grains: it only ſeparated the mercury from the gold. The ſame acid had as little action on the platina grains blackened by calcination, and ſubjected to the ſame ebullition: they retained their cruſt, and the ſtate of the acid was in no manner changed. Margraf and Lewis, however, aſſert that nitric acid has ſome kind of an action on them. But their acid either contained muriatic acid, or the grains on which they operated were more ferruginous. This is very poſſible; for there are ſome kinds of crude platina which have a great many incruſted grains, and others cavernous, which contain red oxide. The black coloured crude platina in my poſſeſſion, and which belonged to Don Pedro d'Avila, is in that ſtate. I can now entertain no doubt that black kinds of platina are ſometimes brought to us. That ſent a few years ago to the king's cabinet, of which I have a ſpecimen given me by our tranſlator of Buffon, is of this kind. It may be whitened by means of the muriatic acid, but not ſufficiently to acquire the brilliancy of the white kind uſed in commerce.

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* The author of the Mineralogy of Spain, Don Chriſtian Herrgen, expreſſes himſelf thus in the ſeventh number of the *Annales de Ciencias Naturales*: "After having read the memoir publiſhed by L. Prouſt in the firſt volume of this collection, one is obliged to acknowledge that platina is never found pure in a native ſtate; it always exhibits a real mineralization, as this memoir proves. It is much to be regretted that we have not yet the leaſt geognoſtic notice reſpecting this valuable ſubſtance. Don Herrgen indicates at the ſame time the places where platina is found, which are, *Choco*, *Popayan*, *Quito*, *Rio del Pinto* in the province of Peru, *Carthagena*, and *Barbuda* in New Grenada.

VII. *Of the Effects of the Nitro-muriatic Acid on Platina.*

I compoſed this acid with the nitric and muriatic acids. I ſhall mention hereafter the proportions beſt calculated to effect this ſolution.

Four hundred grains of crude platina well picked, which had been tranſmitted to me by Don Caſimir Ortega, being treated in this acid until it had loſt all its action, there remained a black matter, which, when waſhed, and collected together in a china cup, was found to weigh 10½ grains, or 2 pounds 10 ounces per cent.

The ſame quantity (400 grains) of crude white platina, which I received from Don Domingo Fernandez, left ſix grains of a black reſiduum, or one and a half per cent.

An equal quantity of the ſame kind, of which the largeſt particles were ſelected, left 14 grains, or 3½ per cent., of a black reſiduum. Theſe three experiments give a mean product of about 2½ per cent.

VIII. *Of the crude Black Platina.*

Four hundred grains of the platina of Don Pedro d'Avila left a reſiduum of 7 grains, or 1 pound 12 ounces per cent.

Four hundred grains of the ſame left a black reſiduum of 9 grains, or 2 pounds and a quarter per cent.

An equal quantity of the largeſt particles of the ſame left a reſiduum of 3 per cent. Thus the mean product of the black kind was 2 pounds 12 ounces per cent.

It is here ſeen that there is a very remarkable variation in the quantity of theſe reſiduums: it ariſes, firſt, from the very variable action of the ſolvent, which more or leſs deſtroys the ſubſtance of the reſiduum; and, in the ſecond place, from there being ſometimes found in theſe reſiduums particles of that metallic ſand of which we have ſpoken, and which are generally pretty large, and imbedded, as it were, in the heart of the platina. This fact has been obſerved alſo by Guyton.

IX. *Of the Graphite of Platina.*

The reſiduums of which we have ſpoken are nothing elſe than graphite or plumbago: it burns ſlowly by the blowpipe, inflames nitre, and deprives the arſenic acid of its oxygen; it leaves ſhining or gloſſy traces on paper; when expoſed to heat, it loſes neither its ſoftneſs to the touch, nor its property of ſtaining: in a word, it has as much luſtre as the beſt Engliſh pencils *.

That the aſſay by this method may be exact, the graphite, when heated by the blowpipe, ought to exhale no ſulphurous odour; for, if this odour is perceived, it indicates that it ſtill contains metal not diſſolved. I muſt however obſerve, that during the ſolution the platina appears to be reduced to a black powder; or, rather, a powder is detached which reſembles galena coarſely pulverized.

This powder, which is as heavy as platina, accumulates at the bottom of the retort: after the ſolution of the grains of the metal, it ſtill retains for a long time the ſame appearance; and it is only by renewing the nitro-muriatic acid that it diſappears entirely, leaving nothing in its place but graphite. The nature of this powder will be ſeen hereafter.

X. *Obſervations on Solutions of Platina.*

To free theſe ſolutions from the nitric acid employed in exceſs to oxygenate the muriatic acid, they muſt be concentrated till they acquire the conſiſtence of honey. The ſuperabundant muriatic acid, becoming oxygenated in its turn at the expenſe of the remaining nitric acid, acquires the quality of oxygenated muriatic gas combined with nitrous gas.

The ſaline maſs which remains is of a dark brown colour: it diſſolves in warm water if the deſiccation has not been too ſtrong; for in that caſe the part which reſiſts ſolution is a brown muriate containing leſs acid, in which platina, iron and copper are re-found, and from which the muriatic acid cannot be ſeparated even by ſeveral waſhings. In regard to the liquid ſolution, it is merely a muriate; for, when decompoſed by means of potaſh, nothing is produced but muriate of potaſh and a little ſulphate. It has an aſtringent taſte, but without any of thoſe metallic ſavours which are ſo diſagreeable in all ſolutions of ſilver, copper, &c.

If the ſolution in this ſtate be treated with potaſh, the ſaline precipitates common to all ſolutions of platina will be obtained, but not an atom of nitrate will be obtained by evaporation; from which it may be inferred, that the nitric acid of ſolutions not concentrated has no part in precipitations of this kind. It will be ſeen, in the courſe of this paper, that the ſlighteſt veſtige of this acid is not found in the precipitates of the moſt nitric ſolutions. In a word, the reſult of ſolutions of platina in nitro-muriatic acid is, as in that of gold, a pure and ſimple muriate of platina. To what has been ſaid I ſhall add, that the difference between one kind of crude platina and another may be known by that obſerved in the colour of their ſolutions. Black platina grains, for example, diſſolved in the ſame quantity of acid as the white, give a colour much redder.

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* On fuſing crude platina with oxygen gas, a black ſhining ſcale forms on the outſide of the globule, which may be ſeparated by the hammer. This is frequently found to ſoil the fingers.—Edit.

XI. *Solution of crude Platina on a large Scale.*

The phænomena obſerved in the ſolution of this mineral, when large quantities are employed, do not differ from thoſe effected by ſmall quantities. In the firſt of theſe operations, however, there are ſome effects more ſtriking, which I ſhall here point out.

When the nitro-muriatic acid, aided by a moderate heat, begins to attack platina, the firſt bubbles of gas become covered with a yellow powder, which retains them at the ſurface of the liquor. Theſe globules float in every direction, and at length adhere to the ſides of the retort, or vaniſh, to give place to others. Theſe phænomena being common to many ſulphurous metals, we are authorized to believe that this ariſes from the ſulphur which envelops theſe bubbles of gas. This powder, when thus depoſited on the ſides of the retort, is ſoon diſſipated; becauſe, being very minute, and in ſmall quantity, it is ſpeedily ſubjected to the action of agents which tend to convert it into ſulphuric acid.

But to return to the black powder: in proportion as the ſolution advances, this powder is ſeen forming itſelf, increaſing, and aſſuming, with the remainder of the platina, a leaden colour, ſimilar, as already ſaid, to that of pulverized galena. It is then proper to lay aſide this reſiduum, on which the ſolvent has only a very weak action, which cannot be continued but by repeated ebullition, and adding a large quantity of new acids. This ſolution, when poured on a new quantity of mineral, acts with a great deal of force; which proves that this reſiduum, though it ſtill holds abundance of metal, reſiſts the action of the ſolvent much more than freſh grains.

This powder has been remarked, but not examined, by thoſe who have operated on platina. To obtain it ſeparately, the reſiduum muſt be waſhed in a great deal of water; it muſt be agitated a great deal, and decanted very ſpeedily, becauſe its gravity is very little different from the mineral which remains to be diſſolved. What has not been carried off by decantation, when moiſtened, and rubbed between the fingers, breaks, and ſtill gives a pretty large quantity of black powder. It is therefore neceſſary to again take from the bottom of the water the mineral found there; to rub it ſtrongly between the hands; to ſeparate the powder from it; and to unite it to the former. It is evident, from what has been here ſaid, that the friction and ſhocks experienced by the mineral, agitated by ebullition, and corroded by the acids, effect the ſeparation of the black powder.

If theſe grains, inſtead of being pounded, are left whole, and if they are examined with a magnifying glaſs, it is ſeen that they have retained their form, that they are eaſily broken, and that they are hollow, and covered with brilliant laminæ. The aperture through which the ſolvent has penetrated into the interior is often diſcovered; and as their colour is in nothing different from that of the powder, there can be no doubt that the latter is the reſiduum of the eroſion of the grains of the platina. In a word, theſe phænomena are more remarkable in black platina ore than in white, becauſe the principle which produces this powder appears to me to be more abundant in the former.

XII. *Experiments on the Black Powder.*

Two hundred grains of this powder, well preſſed together, being calcined in a wide crucible, exhaled ſulphuric acid fumes. Towards the end of the calcination I thought I perceived that the fumes, though ſtill acid, had no longer a ſulphurous character. The reſiduum was pretty black, and began to be agglutinated together: it had loſt 23 parts. The 177 which remained were diſſolved in nitro-muriatic acid; and the ſolution being precipitated by muriate of ammonia, the reſult was an ammoniacal muriate ſimilar to others, of which I ſhall ſpeak hereafter. The remaining liquor was found much leſs coloured than uſual. I treated it with potaſh, which, to my great aſtoniſhment, precipitated a white phoſphate of iron. I call it white, becauſe it is that which all iron oxidated to its *maximum* produces. We have here, therefore, on the one hand ſulphur, and on the other phoſphorus, combined in the reſiduum of our crude platina in greater quantity than in the metal itſelf.

As the remainder of the black powder weighed no more than 42 grains, it is evident that the acid had diſſolved 135 of the 177 grains of black powder calcined.

Theſe 42 grains of powder were ſtill very black: they no longer emitted any odour by the flame of the blowpipe. They were not attacked by the nitro-muriatic acid. They were nothing elſe but plumbago or graphite very pure. To aſcertain with more certainty the nature of this powder, I thought proper to make the following experiment.

I diſſolved 200 grains of black powder well dried, but not calcined, in nitro-muriatic acid of the ſame ſtrength. Having waſhed and weighed the reſiduum, it indicated that the ſolvent had taken up only 24 grains. The ſolution being then treated with muriate of ammonia gave me common muriate of ammonia; and the laſt liquor produced, by the addition of potaſh, phoſphate of iron, which gave me no new information.

But, if it be recollected that 200 grains of the black powder, which had loſt 23 grains of ſulphur and phoſphorus, afterwards ſuffered to be taken up by the ſame ſolvent even 135 grains of platina, one will certainly be aſtoniſhed to find that 200 grains of the ſame powder, which ſtill retained its 23 grains of combuſtible matter, ſhould have reſiſted the action of the ſolvent to ſuch a degree as to give up to it only 24 grains of metal. This reſiſtance muſt neceſſarily find its meaſure in the difference between the numbers 135 and 24, which here repreſent to us the quantities taken from the black powder during the firſt and ſecond experiments.

I then ſuſpected that the union of the combuſtibles accumulated in the platina, formed, perhaps, the greateſt obſtacle to the ſolution, both in theſe experiments and in thoſe made on platina on a large ſcale. Several facts, which I had already obſerved in regard to the ſolution of metallic ſulphurets, compared with that of their reſpective metals, confirmed me in this opinion, at leaſt until other obſervations had enabled me to rectify what might not be founded on that conjecture.

I therefore collected the powder, after the ſmall loſs it had experienced in the nitro-muriatic acid, and expoſed it to the flame of the blowpipe. At the ſame inſtant it exhaled fumes of ſulphur, and perhaps even of ſulphuric acid, as denſe as that which it had produced before being ſubjected to the action of the ſolvent. If we ſuppoſe, then, that the black powder gives up to the ſolvent 135 parts inſtead of 24, at the moment when it is ſeparated from the combuſtibles by torrefaction, there remains no doubt that theſe combuſtibles, on the one hand, oppoſe a ſtrong reſiſtance to the action of acids, and defend the platina from it; and, on the other, that œconomy requires, in operations on a large ſcale, that the black powder ſhould be preſerved ſeparate, to be calcined before it is treated with nitro-muriatic acid.

If we reflect that the ſulphurets and phoſphurets of iron, which are the only ores found in ſmall quantity in the mineral, are incapable of oppoſing ſuch a reſiſtance to the action of the nitro-muriatic acid, it will no longer be doubted that it is chiefly owing to the ſulphurated phoſphorized platina.

In ſupport of my conjecture, I ſhall ſay a few words reſpecting the ſulphurets of which I have ſpoken.

The ſulphuret of mercury, or cinnabar, contains the metal in its natural ſtate, and not, as is commonly ſaid, in that of oxide; for, notwithſtanding the diſpoſition of mercury to unite with oxygen, concentrated nitric acid does not attack cinnabar.

Copper takes up 28 parts in 100 of ſulphur, and never leſs: it can, however, diſſolve ſulphuret of copper in various proportions, as may be ſeen in my eſſays on black copper. If copper containing ſulphuret be expoſed to nitrous acid of 12 or 15 degrees *, the copper will be diſſolved; and ſulphuret, which will ſeparate itſelf under the form of a blackiſh blue powder, cannot be oxidated but in a very ſtrong heat.

I applied ſeveral times an acid of 35 degrees † to American ſilver ore containing ſulphurets of copper, lead, iron, antimony, zinc, mangan eſe, &c., and found that, if theſe mixtures were not heated, the greater part of the metals were attacked by the acid, the whole, or at leaſt the greater part, of the ſilver in the ſtate of ſulphuret excepted; from which we may conclude that this ſulphuret is one of thoſe which reſiſt the action of the nitric acid much more than the metal itſelf. It is very probable that this is the caſe with many others, which I do not at preſent recollect.

[To be continued.]

IX. *On Bleaching. By a Correſpondent.*

To the Editor of the Philoſophical Magazine.

SIR,

THE importance of the new mode of bleaching to the principal manufactures of this country renders it worthy the moſt minute and accurate attention. Before your publication of O'Reilly's Eſſay, no complete treatiſe on the modern ſyſtem had appeared. That of Berthollet, the diſcoverer, though in many particulars very defective, was undoubtedly the beſt adapted to inſtruct the practical manufacturer, who cannot be ſuppoſed intimate with the minutiæ of the laboratory. The in-definite eſſay of Higgins preſuppoſes too much previous knowledge in the reader, ever to enable the inexperienced to adapt or eſtabliſh the new proceſs; and the many other detached eſſays *, however valuable on particular parts, are yet far from forming a *whole*, whence the practical bleacher might gain inſtruction to adapt or improve this new ſyſtem. The preſent eſſay not only preſents a complete view of all that has hitherto been publiſhed, but adds many original facts to the illuſtration of the *modus operandi*, or theory and practice of bleaching. It is therefore with no inconſiderable deference that I venture to offer ſome remarks on the work of ſo experienced a chemiſt.

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* If theſe are degrees of Baumé's areometer, as may be preſumed, they indicate a ſpecific gravity of from 1·089 to 1·114.

† Specific gravity of 1·314.

That there ſhould ſtill be ſuch a diverſity in the quantity of the ingredients for preparing the oxymuriatous acid may appear ſtrange, when its nature and properties are ſo well underſtood. This may be accounted for only by the peculiar modes of preparing it, for the end of all is the ſame. The firſt attempts to prepare this acid fit for bleaching often fail, and the cauſe of ſuch failures, owing, perhaps, only to the want of manual dexterity, is generally aſcribed to a deficiency in the quantity of ſome of the ingredients. To remedy this ſuppoſed deficiency, an unneceſſary quantity of ſome one article is added: hence the German practice of uſing 64 parts ſalt to 20 manganeſe, when moſt probably the converſe quantities, properly managed, would have much ſooner produced the deſired effect. The uſe of ſalt in this proceſs has, no doubt, hitherto been greatly over-rated. It does not appear that muriatic acid alone poſſeſſes any bleaching powers, but that of depoſiting a pale ſubſtantive yellow upon every other colour. To oxygen, of which it is only the vehicle, is juſtly aſcribed the deterſive property of oxymuriatous acid; and ſtuffs are whitened with greater or leſs celerity, according to their greater or leſs facility of combining with oxygen. The colouring matter undergoes a real ſlow combuſtion, which terminates by the formation of carbonic acid, which, eſcaping under the form of elaſtic fluid, produces what we call *bleaching*. Granting that combuſtion actually takes place, (in which there is no leſs of theory than experiment,) and the oxygen, uniting with the carbon, eſcapes in the ſtate of carbonic gas, the muriatic radical and baſe of the colouring matter muſt unite and form a concrete ſubſtance; the reſiduary product of combuſtion, which literally dyes a leaden white, and which, like leaden paint, on expoſure to the atmoſphere, ſhortly aſſumes a dirty yellow colour. That this is really the caſe can ſcarcely be doubted; and that all the attempts hitherto have done no more than dyeing a leaden white, which ſoon becomes a pale yellow, that requires the joint efforts of oxygen and alkalies to remove, muſt, on a moment's reflection, be evident to every one. To this cauſe may be aſcribed the failure of the learned editors of the Encyclopædia Britannica, who were no mean chemiſts. With the acid prepared according to Berthollet's mode they dyed yellow, but could not bleach white.

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* It is poſſible ſome of theſe may have eſcaped the writer's attention.

It is generally agreed, that the muriatic acid is no further uſeful to bleaching than by its power of retaining oxygen in water, and thus conveying it in a ſtate fit to act, more or leſs, on both the external and internal parts of the ſtuff to be bleached. Hence it is evident, that the uſe of ſuch a quantity of ſalt is not only injurious to the celerity of the proceſs, but entails an embarraſſing expenſe on this method of bleaching. However, even this more than triple quantity of ſalt may have been neceſſary to incorporate or retain the requiſite quantity of oxygen to perform the whitening proceſs, if the apparatus were not properly adapted. Condenſers have been generally uſed, and Mr. O'Reilly propoſes (Plate I. fig. 1. C.*) an *improved* one; but, if duly examined, they will be found not only inefficacious, but highly diſadvantageous to the admixture of the oxygen with the water. It is well known how ſlight the union of oxygen with muriatic acid is; and whoever will try even this improved condenſer will find an almoſt complete ſeparation of the two gaſes before they are brought in contact with the water. So ſlightly indeed are they mixed, that the moment the heat and conſequent motion is withdrawn from theſe condenſers, that moment the gaſes begin to ſeparate according to their ſpecific gravities. At the top will be found tolerably pure oxygen gas, while the bottom contains muriatic mixed with ſome ſulphuric acid gas. This laſt combination is conſiderably deterſive, but is alſo ſomewhat corroſive, and has often damaged goods; a circumſtance that may be avoided by having them previouſly wetted in water. It is leſs injurious, however, to cotton than linen goods; and to this is owing probably the great quantity of ſulphuric acid uſed at Mancheſter, where it is diluted with double its quantity of water; which muſt excite ſuch a rapid ebullition, that ſome of the ſulphuric acid will be carried into the receiver. Sulphuric acid, diluted with *leſs* than its volume of water, will produce the pureſt oxymuriatic gas. By uſing condenſers, much of the oxygen too is diſſipated through the anti-abſorbent tube (if I may ſo call it) at the top of the condenſer. Under ſuch circumſtances it is not ſurpriſing that extraordinary quantities of muriatic acid ſhould be required to retain a very ſmall quantity of oxygen.

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* See Philoſophical Magazine, vol. x.

To obviate the loſs of the oxygen, and prevent the neceſſity of uſing ſo much ſalt, the alembic and great receiving vat, or vats, ſhould always communicate immediately together, without the intervention of condenſers, which always tend to diminiſh the capability of the muriatic acid to unite with and retain the oxygen in the water, and thus defeat the object of the proceſs. Abſorption might be guarded againſt by having the communicating tube bent, like a double ſyphon (♑), and in the lower angle to have an air-tight ſtopper, that would yield on the firſt preſſure, ſo that no water could ever reach the alembic. It ought to be remembered too, that if by neglect abſorption ever takes place, the liquor will be rendered almoſt totally inert, even with the beſt condenſers. Various other modes of anti-abſorption might be adapted equally efficient with thoſe in uſe, and without their inconveniences.

When citizen Paul thinks proper to publiſh his mode of impregnating water with oxygen, perhaps a ſtill more ſpeedy and effectual method of diſcharging the colouring matter of cotton and linen may be found; and the theory or proceſs of combuſtion, adapted by Berthollet in his "Elements of the Art of Dyeing," and ably combated by Dr. Bancroft* in his moſt ingenious "Reſearches in the Philoſophy of permanent Colours," be ſuperſeded by the more ſimple term *ſolution.* **J. A. B.**

Tokenhouſe-yard,
Sept. 9, 1801.

X. *Experiments on Dyeing Cotton with the Flowers of the Carthamus Tinctorius* (Safflower). *By Profeſſor* BECKMAN†.

ABOUT ſeven years ago, I made various experiments on dyeing wool with the flowers of the carthamus; and two years after I added ſome experiments on dyeing linen in the ſame manner‡. As theſe were approved by men well verſed in that part of chemiſtry which relates to the art of dyeing,

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[*] May it be permitted to aſk this philoſopher, through the medium of the Philoſophical Magazine, when the world will be favoured with his promiſed ſecond volume of Reſearches?

[†] From the *Tranſactions of the Royal Society at Göttingen for the Year* 1780.

[‡] Novi Commentar. Societ. Göttin. iv. p. 89. and vi. p. 79.

I reſolved to make ſome experiments, alſo, on dyeing cotton with the flowers of the carthamus; eſpecially as no perſon, as far as I know, had ever before ſhown what effect might be produced by the beautiful red dye of this flower.

The celebrated Poerner [*] made, indeed, various experiments on cotton; but his only relate to the ſaffron-coloured dye of the plant; nor did he employ any alkaline ſalt to extract the red dye. Scheffer [†] alſo made experiments on the ſame ſubject; but my obſervations have not been anticipated by his, ſince he tells us, that the method of dyeing cotton and ſilk with theſe flowers is the ſame, though it may be eaſily proved that there is a very eſſential difference. But, before I give an account of my experiments, let me be permitted to make a few obſervations reſpecting the art of dyeing cotton in general.

No doubt can be entertained that it is more difficult to dye cotton than to dye wool or ſilk. Every thing made of wool readily imbibes the dye: ſilk imbibes it with more difficulty: but cotton rejects all dyes ſo much, that thoſe which immediately penetrate wool, and tinge it with a ſtrong colour, ſcarcely communicate any tint whatever to the latter, and even the dye which adheres to it may be eaſily diſcharged, and oft-times by water alone. For this reaſon, there is ſcarcely any colour on cotton, except that called the Turkey red, which is either agreeable to the ſight or of a durable nature. But, if we wiſh to inquire into the cauſe of this difference, we muſt attend to the nature of the two ſubſtances.

There can be no doubt that wool contains a larger quantity of fat, or inflammable matter, than cotton. We have reaſon likewiſe to conjecture, that wool, and the hair of animals, have more and larger pores, as well as more capacious fibres, than thoſe ſubſtances procured from vegetables. The great object, then, will be, to remove by art that difference which there is between animal and vegetable ſubſtances. That the ſmallneſs of the pores or fibres can be corrected by art, we have ſcarcely reaſon to hope: but we may make choice of colouring ſubſtances well diluted and ſoft, and as fluid and penetrating as poſſible, that is, rather ſaline than earthy; and may alſo prepare or macerate the cotton in ſuch liquors as are capable of rendering its fibres ſofter and looſer. Daily experience, however, ſhows that much cannot be effected in this manner; for though cotton, by means of ſalts, may be made to imbibe various dyes, yet they are eaſily waſhed out and deſtroyed. Let us try, then, if a method cannot be diſcovered to render the nature of cotton more like to that of wool. This, indeed, will be the caſe, if we inſtil into it an oily ſubſtance ſo as to become a component part of it, which can neither be waſhed out with cold water nor ſoap, nor in any other manner.

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[*] Poerner Färberey-Verſuche, vol. iii. p. 138.

[†] Scheffer Chemiſche Vorleſungen uberſezt von Weigel, p. 675.

But as the Turkiſh red poſſeſſes all thoſe qualities which are requiſite in ſtuffs that have been dyed, this affords a ſufficient proof that cotton may be ſo prepared by art as to receive a conſtant and fixed colour. That Turkiſh dye, therefore, muſt be the pattern to which all our experiments are directed: for it ſtands the rays of the ſun, as alſo the action of ſoap and common ley; though when often waſhed, and expoſed to the ſun, it undergoes ſome variation; but the colour is ſo far from becoming worſe, that it is rather improved, and rendered brighter. If the cotton yarn, however, be digeſted long in a cauſtic ley, or rubbed in it too much, the colour is injured. Rectified ſpirit of wine (alcohol) does not hurt the colour of the yarn digeſted in it, nor does the ſpirit acquire any tincture from it. Spirit of ſal-ammoniac, prepared by means of lime (cauſtic ammoniac,) is no leſs ineffectual. The colour is not injured by urine when undergoing decompoſition; nor does microcoſmic ſalt hurt, but rather makes the colour more agreeable, as is ſaid; though that experiment I never tried. Mineral acids corrode the threads, but the colour itſelf remains. If digeſted for ſome days in nitric acid, or in *aqua-regia*, the colour becomes yellowiſh, and at length diſappears.

Theſe experiments, which I myſelf and many chemiſts have tried, ſeemed to preclude all hope of diſſolving and diſcovering the nature of that dye uſed by the Turks, till J. C. Oettinger, formerly profeſſor at Tubingen, who has deſerved ſo well of chemiſtry, threw out ſome hints likely to lead to a knowledge of it. He, as far as I know, was the firſt perſon who had an opportunity of obſerving the great effect which oil of olives has upon that Turkey dye. He found, that if cotton yarns dyed with it were dipped in this oil, and then rubbed a little, they loſt their colour, and that the whole colouring matter became ſo thoroughly diſſolved, that other threads could be dyed in that oily ſolution; ſo that, by the help of oil, the dye on Turkiſh yarn could be transferred to other undyed cotton.

Theſe obſervations afford grounds for various conjectures. The colouring matter which the Turks uſe, is perhaps of ſuch a nature as to be ſoluble in oil in the ſame manner as reſins; and perhaps the colouring matter adheres to vegetable

table ſubſtances by means of ſome matter diſſolved by the oil. But the conſtancy of the colour in alkaline ſalts oppoſes this conjecture; and it is more probable that the Turks may have diſcovered ſome method of aſſimilating cotton ſtuffs to thoſe made of wool, and of thus rendering them more capable than they naturally are of receiving the colouring ſubſtance.

The celebrated profeſſor Pallas proved, a few years ago, that this conjecture is not only probable, but even true. This gentleman, who, in travelling through the immenſe provinces of the Ruſſian empire, directed his attention not only to obſerving and collecting animals, plants, and minerals, but alſo to various uncommon proceſſes employed by the inhabitants, which might ſerve to improve the arts of Europe, was ſo fortunate as to acquire a knowledge of the manner in which the Armenians, who have retired to Aſtracan on account of the continual troubles in Perſia, dye cotton after the Turkiſh manner. This proceſs, which the profeſſor communicated to the public in 1776 in the Peterſburgh Journal, I ſhall not deſcribe at full length, but only relate a few circumſtances that may ſerve to confirm my opinion, and to be a guide to others in proſecuting experiments on this ſubject.

The cotton yarn is ſteeped by the Armenians in ſiſh oil. It is then dried in the open air, and this proceſs is repeated for ſeven days and nights. The Armenians ſay that no ſiſh oil can be uſed but that which aſſumes a milky appearance by the addition of an alkaline ſolution. I, indeed, ſuſpect that the fat of marine animals, and, in particular, ſiſhes, is much ſofter and more ſubtle, and penetrates ſolid bodies much more eaſily than the fat of other animals; and it is for this reaſon, in my opinion, that the Armenians uſe only ſiſh oil, which can be readily procured at Aſtracan.

The yarn which has imbibed a ſufficiency of oil is waſhed, dried, and dipped in an aſtringent liquor in which there is an abundant mixture of alum.

Cotton yarn thus prepared is dyed with the colouring matter of madder which has been mixed with calves blood. In my opinion, the dye of the madder is not only heightened by the redneſs of the blood, but, as the red part of the blood is of an oily nature, as is proved by the red globules, which, by the help of a microſcope, are ſeen floating on blood, it may facilitate the entrance of the colouring matter into the pores of the yarn, and the alkaline ſalt contained in blood acts ſtrongly on the colouring particles of the madder, which it extracts, as I have already proved by experiments with alkaline ſalts.

When the yarn is dyed, it is digeſted for 24 hours with a ſtrong ſolution of ſoda in an earthen jar placed over a fire. By theſe means the colour is more ſtrongly ſaturated, as the alkaline ſalt renders moſt red colours more obſcure or yellow; and, if there are any remains of oil, they are entirely extracted by the ſolution of ſoda *.

What has been here ſaid, is confirmed by the accounts given of the method of dyeing employed in the Eaſt Indies; for, though it may appear ſomewhat different from that followed at Aſtracan, yet the dyers there make uſe of animal oil or animal ſoap, which adheres to the dung of ſheep and other animals †.

Though there is no doubt that the method of dyeing deſcribed by profeſſor Pallas might be ſucceſsfully imitated by our dyers, it is not my intention to make any trial on madder, but rather to apply the principles, which I have already explained, to the uſe of the carthamus. For this reaſon, I ſubjected to experiments a piece of cotton cloth commonly called *chintz*, which was white, and had never been worn. I waſhed carefully the flowers of the carthamus, that nothing of their yellow colouring matter might remain; after which I ſteeped them in an alkaline ſolution, and expreſſed the juice. This expreſſed juice, which contained all the red dye of the carthamus, and which I ſhall call the *alkaline liquor*, I tempered with the acid of lemon juice.

1. Having melted hog's lard, I ſteeped the cloth in it for two hours, and then waſhed it in cold water until ſcarcely any reſiduum of the fat ſubſtance was ſeen floating on the ſurface. I then ſteeped the cloth, after it was thoroughly dried, in the alkaline liquor, and, when it was well ſaturated, I threw it into lemon juice, and repeated the ſame proceſs frequently. I proceeded in the like manner with chintz, which I had premacerated in water alone, and not with animal fat; but the latter imbibed much leſs of the colouring matter: and this experiment, therefore, proves the advantage of uſing fat ſubſtances.

2. In the like manner, I premacerated cotton cloth in a ſolution of common ſoap, and, after it had boiled for ſome hours, I dried it. This experiment alſo ſucceeded; for the

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* For profeſſor Pallas's account of the proceſs followed at Aſtracan for dyeing Turkey red, ſee Philoſophical Magazine, vol. i.

† Compare *Lettres édifiantes*, rec. xxvi. See alſo *Mémoires de Mathématique et de Phyſique preſentés à l'Academ. de Paris*, tom. iv. p. 1. The proceſs for dyeing deſcribed by Pallas is ſimilar to that inſerted in *Obſervations ſur le Commerce et ſur les Arts*, par Flachat, tom. ii. p. 405.

cloth thus prepared was much better dyed than that ſoaked in water alone.

3. But common oil of olives is much ſuperior to either hog's lard or ſoap. I boiled cloth in this oil for three hours; I ſqueezed out the oil, and waſhed the cloth in cold water till not a drop of oil ſeemed to be left in it, though the ſmell of it remained. I then dried the cloth either above a ſtove or in the open air; dipped it ſeveral times, alternately, in the alkaline liquor and lemon juice; and obſerved that there was a very eſſential difference between the cloth which had, and that which had not, imbibed the oil. The one had aſſumed a ſtronger and much better ſaturated colour; and the other one much weaker, and was, at the ſame time, covered in many places with various pale ſpots. The cloth, alſo, which had not yet imbibed oil, loſt much of its colour in drying; but the colour of the other in drying ſeemed to acquire ſtrength. I learned alſo, by experience, that the whole proceſs ſucceeds better, the oftener the cotton is thus dipped in oil and dried. It is certain, alſo, that the cloth extracts ſome colour from the oil, and therefore it is neceſſary that the latter ſhould be pure and limpid. The alkaline ſalt, by which the colouring matter of the carthamus is extracted, is of great ſervice in this proceſs; for the oil is ſo far from rejecting the watery colouring matter, which ſome might apprehend, that it rather ſtrongly attracts it, on account of the alkaline ſalt, which facilitates its entrance into the pores of the cotton.

4. As theſe experiments had ſucceeded, I endeavoured to imitate with the carthamus, as far as poſſible, the proceſs employed by the Armenians. With this view, I boiled a piece of cotton cloth ſeveral times in olive oil; then waſhed it thoroughly in cold water, and afterwards dried it. After this, I mixed water impregnated with the yellow colouring matter of the carthamus, or a yellow infuſion of the carthamus, with pounded galls and alum. I then took cloth which had been premacerated in oil, and dipped it in this ſolution, after it had boiled a little, and found that the cloth, when wrung, was of a yellow colour. Having dried the cloth, I dipped it in the cold alkaline liquor of the carthamus, and then immerſed it in lemon juice; and this being frequently repeated, the cloth appeared of a beautiful and full red. Cloth which had not been ſteeped in oil, but which in other reſpects had been expoſed to the like proceſs, was of the ſame colour, but a little paler; and, for that reaſon, I recommend this mode of dyeing to thoſe who do not chooſe to employ oil.

I ſhall now add in what manner I tried the conſtancy or durability of this dye. But I ſhall ſpeak only of thoſe cloths which were premacerated in an aſtringent ſolution of alum. The cloth which had imbibed oil, and that which had not imbibed it, both ſtood alkaline ſalts; for theſe ſalts rendered the colour of both darker, without any other change. Nitric acid not diluted with water rendered the colour of both firſt brighter, then yellow, and at laſt deſtroyed it altogether. But ſtill there was a great difference: for the cloth which had been prepared with oil retained its colour much longer, and when waſhed in water, at the end of ſeven or eight minutes the colour returned, but of a violet hue; whereas cloth prepared without oil loſt its whole colour ſooner. The ſame acid diluted injured the latter, but not the former. Even the Turkey dye, prepared with madder, which is more conſtant, will not long withſtand the nitric acid; nor is it neceſſary that all our cloths ſhould be of ſuch a nature as to bear all acids. If a few drops of mineral acid fall by chance upon a piece of cloth dyed in this manner, a yellow ſpot is produced; but it entirely vaniſhes from both kinds, if the acid be immediately well waſhed out with water. It is needleſs to remark, that acid vegetable juices do this dye no hurt. It ſuſtains no injury from human urine, even if the cloth be left in it for ſeveral days. When expoſed to the rays of the ſun for ſome days, theſe cloths ſuffered no injury, though that dyed without oil ſeemed to have loſt more of its colour. But cloth which has even been prepared with oil, will not bear to be boiled with ſoap; its colour firſt becomes violet, and then totally diſappears.

Theſe few experiments ſo fully demonſtrate the great advantage of oil in dyeing cotton, that no doubt can be entertained of its utility. I muſt ſtrongly recommend it, therefore, not only to thoſe who dye cotton, but to thoſe alſo who dye linen. For, as the great power of the oil on the too fading dye of the carthamus is, that the colouring matter is ſooner abſorbed, and in greater abundance, by the cloth, and alſo retained longer by it, its power would doubtleſs be much greater upon dyes which, by their natural qualities, bear, without any aſſiſtance, ſoap, the rays of the ſun, and ſalts, by which the dye of the delicate carthamus is injured. I do not, however, imagine that I have found out the beſt method of employing oil; but I entertain a hope, that thoſe who may proſecute theſe experiments will diſcover a better. Thoſe, in my opinion, will be more ſucceſsful who can uſe fiſh oil, which, in many places, may eaſily be procured by dyers. Nor is it impoſſible that whale oil, called commonly train oil, may be ſo prepared as to be fit for the purpoſes of dyeing. Dyers, however, may reſt aſſured that the colour which they wiſh for will be more conſtant the oftener the cloths to be dyed are dipped in oil, or are boiled in pure ſubtle oil, and then dried.

XI. *Deſcription of a new Furnace for conſuming its own Smoke, and ſaving Fuel: invented by Meſſrs.* ROBERTON, *of Glaſgow.*

TO conſtruct furnaces on ſuch a principle as ſhould enable them to conſume their own ſmoke, has long been a deſideratum; and we believe the public in general, but eſpecially thoſe who have been annoyed by the ſmoke of ſteam engines, founderies, and ſimilar erections in their neighbourhood, will be glad to learn that a furnace has been contrived which effectually gains this end.

The conſtruction is extremely ſimple, and will be eaſily underſtood by the following deſcription, and the plate to which it refers.

Fig. 1. (Plate 1.) repreſents a vertical ſection, and fig. 2. a front view of a ſteam-engine boiler, furniſhed with one of Meſſrs. Roberton's furnaces; and the ſame letters refer in both to the ſame parts of the conſtruction.

The opening A, through which the fuel is introduced into the furnace, is ſhaped ſomewhat like a hopper, and is made of caſt iron built into the brickwork H, H. From the mouth it inclines downward to the place where the fire reſts on the bottom grate B. The coals in this mouth-piece or hopper anſwer the purpoſe of a door [*], and thoſe that are loweſt are by this means brought into a ſtate of ignition before they are forced into the furnace. Below the lower plate of the hopper K, *e* the furnace is provided with front bars G[†], which not only ſerve to admit air among the fuel, but offer a ready way to force the fuel back, from time to time, from *c* to *d* [‡], to make room for freſh quantities to fall into the furnace from the hopper or mouth-piece. By this arrangement the fuel is brought into a ſtate of ignition before it reaches the further ſide of the bottom grate, where it is ſtopped by the riſing breaſt, *b*, of the brick-work, ſo that any ſmoke liberated from the raw coals in the mouth-piece muſt paſs over theſe burning coals before it can reach the flue FFF. But this, though it would cauſe a large quantity of the ſmoke to be burnt, would not completely prevent the eſcape and aſcent of ſmoke up the chimney; for it is not merely neceſſary that the ſmoke ſhould be expoſed to a heat ſufficient to ignite it before it eſcapes: unleſs, at the ſame time, a quantity of freſh air, able to furniſh a ſufficiency of oxygen for the combuſtion of the ſmoke, can be brought into contact with it, it will ſtill eſcape in an undecompoſed ſtate. The judicious admiſſion of freſh air, in ſuch a manner that it can reach the ſmoke, without previouſly paſſing through the fire, and parting with its oxygen in its paſſage, and in ſuch quantity as not to cool the bottom of the boiler, but merely to cauſe the ſmoke to burn, conſtitutes the chief merit of this invention; and to us it appears that it will fully anſwer the propoſed end. Below the upper ſide of the mouth-piece or hopper, and at about the diſtance of 3-4ths of an inch from it, (this ſpace being a little more or leſs, according to the ſize of the furnace,) is introduced a caſt iron plate *a n*. This plate is above the fuel, and the ſpace between it and the top of the hopper is open for the admiſſion of a thin ſtream of air, which, ruſhing down the opening, comes firſt in contact with that part of the fire which is giving off the greateſt part of the ſmoke, viz. the fuel that has been laſt introduced, mixes with it before it paſſes over the fuel in the interior, which is in a high ſtate of combuſtion, and enables it to inflame ſo completely, that not a particle of ſmoke ever eſcapes undecompoſed.

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[*] In the management of this furnace, what is chiefly to be attended to is, that the hopper be kept full of coal, and either wholly or in part ſmall coal, to prevent, as much as poſſible, air getting in by that paſſage: it is alſo neceſſary at ſome times to uſe a ſhutter of thin plate-iron, to be applied to the mouth of the hopper to exclude the entrance of air by that paſſage.

[†] Theſe bars are, in fact, a grated door, kept in their poſition by a catch L, and which may be opened at pleaſure for cleaning the fire out. In ſmall furnaces an opening here is all that is neceſſary: the bars may be diſpenſed with.

[‡] Between the back end, *d*, of the bottom bars, and the breaſt brickwork *b*, is repreſented in the plate a ſection of a ſhutter, which is ſometimes opened for the purpoſe of getting out the refuſe of the fuel.

The quantity of air thus admitted to paſs over the upper ſurface of the fire, is regulated by a very ſimple contrivance: The plate *a n* reſts at each end on a ſtud, or pin, projecting from the cheeks of the mouth-piece A, or is furniſhed at each end with a pivot which works in the cheeks; the ſaid pins or pivots being placed about mid-way between the outſide and inſide of the mouth-piece or hopper, ſo that, by elevating or depreſſing the edge *a* of the plate, the opening at *n* is enlarged or diminiſhed. When that degree of opening which produces the beſt effects is obtained, which is eaſily known, the plate *a n* is kept in its place by means of a piece of iron introduced above it, and anſwering the purpoſe of a wedge.

Under the grates is the aſh-hole I, the upper part of which is furniſhed with doors S S, which, when ſhut, prevent the heat from the front bars G from coming out into the apartment, and incommoding the workmen

Invited by an advertiſement, we went to Meſſrs. Bunnell and Silver, Bedford ſtreet, Covent Garden, to ſee one of theſe furnaces at work, and we were not a little gratified in obſerving that the ſmalleſt appearance of ſmoke could not be perceived iſſuing from the top of the chimney. The advantages of ſuch an improvement can hardly be better illuſtrated than by mentioning what had actually happened with this ſteam-engine. The ſmoke, before the improved furnace was employed, incommoded the neighbourhood ſo much, that it was ſtopped as an intolerable nuiſance. Now it is ſo far from diſturbing any one, that, without being admitted to ſee the engine, it would be actually impoſſible to know when it is at work.

Theſe furnaces, we underſtand, have alſo been adopted by many intelligent manufacturers at Leeds and at Mancheſter. At the latter place, if we may credit newspaper reports, ſeveral manufacturers have had their works indicted as nuiſances for not having adopted the improvement; the magiſtrates arguing, that, though the welfare of the place required that ſuch inconveniences ſhould be ſubmitted to while no poſſible cure for them was known, the health and comfort of the inhabitants equally demand, now that the evil can be done away, that ſmoking furnaces ſhould not be permitted in the place.

We earneſtly recommend to owners of ſteam-engines, and alſo to thoſe who are annoyed by them, to endeavour to bring this improvement into general uſe. Indeed, we entertain no doubt of its being univerſally adopted ſooner or later; for it yields advantages not only in point of cleanlineſs, comfort, and health, but alſo in point of *intereſt*—all the ſmoke uſually diſcharged at the top of the chimney, being, in fact, ſo much good fuel, that only wanted the contact of freſh air to inflame it under the boiler. It is a fact well known, that the flame which is often ſeen iſſuing from the chimneys of founders, &c. has no exiſtence except at the top of the chimney: while aſcending the flue it is only denſe ſmoke, conſiſting of the azote of the atmoſpheric air decompoſed in paſſing through the fire, of hydrogen, coal-tar, and carbonaceous matter, of ſuch a high temperature, that it only wants oxygen to make it inflame ſpontaneouſly: this it obtains from the atmoſpheric air into which it aſcends, and then preſents ſuch appearances as would make a haſty obſerver adopt the opinion that the flame had aſcended, as flame, from the fuel in the furnace; which is by no means the caſe. A conſideration of this ſimple fact will convince any perſon that it is not an inconſiderable proportion of the fuel that is thus waſted. Nor is this the only loſs ſuſtained; the quantity of heat required not merely to render ſuch a portion of the fuel volatile, but to give to it a temperature able to produce the effect of which we have taken notice, is itſelf furniſhed at the expenſe of an extra and unneceſſary quantity of fuel! The whole waſte in many caſes is, we are perſuaded, not leſs than an eighth of the whole fuel employed.

XII. *Obſervations on the Onager of the Antients, or the real Wild Aſs. By Profeſſor* PALLAS *.

OUR cabinets of natural hiſtory contain a multitude of animals, and other natural productions, from both the Indies, which are even ſcarce in their native countries, and which, on that account, are ſent as rarities to Europe: on the other hand, many ſpecies common in diſtricts not very remote from Europe are ſtill imperfectly known, and therefore the exiſtence of many of them is accounted doubtful, becauſe people do not give themſelves the trouble to examine the places where they are found. Of the truth of this obſervation, the wild aſs, or onager, ſo often mentioned by antient authors, is a remarkable inſtance. From the ſilence of moſt of the modern travellers in the Eaſt reſpecting it, one might almoſt be induced to conclude that the race of this animal is become entirely extinct. An evident cauſe of this is, that the Europeans have very little intercourſe with thoſe diſtricts from which the Romans procured wild aſſes for breeding mules. As our travellers are obliged, for the ſake of ſecurity, to go in large caravans when they paſs through the Aſiatic deſerts, where theſe animals reſide, they can ſee only a very ſmall portion of the country. It is therefore not ſurpriſing, that animals noted for their ſhyneſs and timidity ſhould eſcape their obſervation. I have been aſſured, in a letter from M. Niebuhr, the celebrated traveller in Arabia, that during the whole courſe of his travels he never ſaw a wild aſs, not even in Syria, where, at the time when that country was viſited by Rauwolf †, who ſpeaks of theſe animals, they muſt not have been very uncommon. Beſides this old naturaliſt, and

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* From *Neue Nordiſche Beyträge*, by profeſſor Pallas, vol. ii.

† Rauwolf's Reiſe, *Augſ.* 1583, 4to. p. 65.

Pietro della Valle *, I ſcarcely know one traveller in the Eaſt who has ſpoken of the onager from his own obſervation. The latter only mentions it in conſequence of having ſeen one which was kept at Baſſora as a curioſity; and Olearius † ſaw ſome wild aſſes in a park in Perſia, where they had been collected for the purpoſes of hunting. One of the oldeſt European travellers, who penetrated to the deſerts of Great Tartary, the monk Rubriquis ‡, firſt makes mention of the Tartarian name *Kulan*, under which the wild aſs is at preſent known by the wandering tribes of theſe countries. In the journal of the two Engliſh travellers Hogg and Thompſon, publiſhed by Hanway § in his Travels, mention is made of whole herds, not only of antelopes and wild horſes, but alſo of wild aſſes, which they met with in the neighbourhood of the lake Aral, and in the courſe of their travels through the diſtricts lying to the eaſt of the Caſpian ſea.

This imperfect information is almoſt the whole of what is found in the modern travellers reſpecting the exiſtence of the wild aſs in Aſia, where this animal, in the time of the Romans, was generally known, as far as Leſſer Aſia, Syria, and Arabia ‖. Reſpecting thoſe in Africa our information is equally deficient; and I can adduce no other teſtimony of their exiſtence in that country, than what is to be found in Leo Africanus and Marmol. For thoſe ſaid to have exiſted formerly in the Canaries in great abundance, were produced from tame aſſes ſuffered to run wild, and are now entirely deſtroyed ¶. The caſe was the ſame with thoſe which, according to Dapper's account, were found wild formerly in ſome of the iſlands of the Archipelago, but of which, at preſent, no traces remain.

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* Voyage de Pietro della Valle, *Amſt.* 1766, part iii. p. 137, part vi. p. 105.

† Olearius Reiſe nach Perſien, *Schleſwig* 1656, p. 526.

‡ Allgemeine Geſch. der Reiſen, vol. vii. b. 17. c. 2.

§ Hanway's Hiſtorical Account of the Britiſh Trade on the Caſpian ſea, vol. i. p. 349.

‖ Varro and Pliny ſpeak of the onager as an animal very common in Leſſer Aſia. Xenophon, Suetonius, and Ammianus Marcellinus, give the ſame account in regard to Meſopotamia, Perſia, and the kingdom of Parthia. Tacitus in his account of the Iſraelites ſays, that during their journey through the Arabian deſert under the direction of Moſes, they often followed the wild aſſes, by which means they were enabled to diſcover ſprings. The Scripture alſo ſpeaks of the wild aſs as an animal very common in the deſerts bordering on Paleſtine. None of the antient authors, however, except Oppian, has left us a preciſe deſcription of the onager; but it clearly appears from Oppian, that under this name the antients underſtood the wild aſs properly ſo called, which I have here deſcribed; and, in my opinion, none but Philoſtorgius applies the above appellation to the zebra.

¶ See on this ſubject the teſtimony of Aloyſius Cadamoſto in the collection of Ramuſio, part i. p. 98: and what Glaſs ſays, in his Deſcription of the Canary Iſlands, of the general hunting match which the inhabitants

By the accounts which I have received from ſome of the wandering Aſiatic tribes, and from Ruſſians and Tartars who eſcaped from the ſlavery of theſe people, and alſo from Bucharian merchants who had travelled with the caravans, it appears certain that in the diſtricts of Great Tartary there are abundance of wild aſſes, which theſe people call *kulan*. Theſe animals proceed from the ſouth in innumerable herds, and ſpread themſelves in the mountainous naked deſerts lying to the north and eaſt of the lake Aral, where they feed during the ſummer, and in autumn unite in hundreds, and even thouſands, in order to ſeek for a warmer winter reſidence in the ſouthern diſtricts on the borders of India and Perſia. From a paſſage in Barboza's Travels * it appears that theſe emigrations extend even to the ſouthern parts of India; but it is certain that Perſia is the moſt common winter reſidence of the wild aſs, and every year they are found in the mountainous diſtricts around Caſbin. Reſpecting their emigrations, in the diſtricts of Tartary, eye-witneſſes have aſſured me that traces of large herds, from two to three hundred fathoms in breadth, may be often ſeen on the plains. But as they remain at a conſiderable diſtance from the Ruſſian frontiers, and ſeldom proceed northwards beyond the 48th degree of latitude, it was never poſſible for me, during my reſidence on the borders, notwithſtanding all my exertions, to obtain a deſcription of this animal from the Kirgiſians. I therefore requeſted profeſſor Gmelin, whom on my return to Aſtracan I found on the eaſtern coaſt of the Caſpian ſea, ready to ſet out on a ſecond tour to Perſia, to procure all the information he could reſpecting the *kulan*, and to make us at length acquainted with the nature of the aſs in its wild ſtate: but he was not able to obtain from the interpreters a real wild *kulan*. We are, however, much indebted to him for two animals of this kind, bred at Caſbin from *kulan* colts which had been caught wild, and which he brought with him from Perſia. The ingenious pupil of Gmelin, who accompanied him on his ſecond tour to Perſia, M. Hablizl, has given a careful deſcription and meaſurement of the *kulan* ſtallion which died in the courſe of the ſea voyage to Aſtracan,

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of the iſland of Forteventura were obliged to undertake on account of the great increaſe of wild aſſes, and during which 1500 of theſe animals were deſtroyed.

* See the account of Odoard Barboza in *Ramuſio*, part ii. p. 300. where wild aſſes are mentioned in the mountains of Malabar and Golconda.

can, together with an accurate drawing of it. The female was brought alive to Peterſburgh, and was delivered to me with the above deſcription; and from theſe the preſent deſcription of the wild aſs or onager of the antients has been formed.

The Perſians pronounce the name of the wild aſs *kurban*, as it is written by Olearius. They call it alſo in the Turkiſh dialect *dagha-iſchaaki*, or the mountain aſs, becauſe it prefers living in the drieſt and moſt mountainous deſerts. Among theſe as well as ſome Tartar tribes it is an object of the chace, and hunted in various ways. The Kirgiſians lie in wait for the *kulan*, and endeavour to ſhoot it in order to feaſt on its fleſh, for the roaſted fleſh of this animal is one of their dainties; and indeed muſt not be ill taſted, ſince we learn from Pliny that the fleſh of the young onager was much ſought after by the Romans*. On the other hand, the Perſians rather try to catch the wild aſs alive in pits carefully covered ſo as to conceal them, which they dig in the ground; and, that the animal may not be injured, they fill the pits to a certain height with hay. The animals are driven towards the places where ſuch pits have been made, by a company of hunters who aſſemble for that deſign, and the young colts are ſold at a high price to the principal men of the country for the purpoſes of breeding. From the wild colts caught in this manner are produced thoſe beautiful and active riding aſſes employed even in Perſia, and in Arabia and Egypt, for travelling through the deſerts, and which in the latter countries are ſold ſometimes for 75 ducats. According to the account tranſmitted to me by M. Niebuhr, there are many of theſe riding aſſes which, in point of colour, have a perfect reſemblance to the wild aſs here deſcribed. Tavernier ſays that, in Perſia, beautiful ſaddle aſſes are dearer than the beſt horſes, and often coſt a hundred dollars†. He makes an expreſs diſtinction between them and the common breed of aſſes, which abound in Perſia as well as in Bucharia and China, but are employed only for the draught and for carrying burdens; and from the Perſian cuſtom which he mentions of painting theſe ſaddle aſſes red, which is done alſo in Egypt with the *kanna*, we may perhaps be able to explain the fabulous account given by Ælian of the red-headed onager in India; to which he likewiſe adds a horn on the forehead [*]. Le Bruyn and Adanſon [†] praiſe alſo theſe riding aſſes produced from wild aſſes, and they are extolled by all oriental travellers. They poſſeſs all the good properties of the wild race, from which they are ſprung; that beautiful figure on account of which Martial gives to the onager the epithet of *pulcher*—great agility and fleetneſs in running. Beſides, they are highly valued becauſe on journeys through the deſert countries they hold out much better than the Tartar horſes, and advance with an uniform pace at a faſter rate than even the camel [‡].

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* *Plin. Hiſt. Nat.* lib. viii. c. 44:—"The beſt onagers are found in Phrygia and Lycaonia. The foals, as dainties, are known under the name *laliſiones*, and come chiefly from Africa." This author ſays ſoon after (cap. 45.) that Mecenas, at the Roman entertainments, introduced the fleſh of young mules inſtead of that foreign veniſon.

† Voyage de Tavernier, liv. iv. c. 3.

The ſhe aſs brought to Peterſburgh, a repreſentation of which in two poſitions is annexed, (ſee Plate II.) had not attained to her full growth, and in all probability continued ſo weakly becauſe ſhe had been caught young and badly taken care of. She, however, was able to perform the journey from Aſtracan to Moſcow, in ſummer, which is more than two hundred German miles, continually trotting behind the poſt-waggon without reſting more than two nights; and though ſhe had ſuffered conſiderably by ſometimes falling down and being dragged behind the carriage, ſhe had ſufficient ſtrength, after a ſhort ſtay at Moſcow, to travel two hundred miles further to Peterſburgh: when ſhe arrived there, indeed, ſhe was exceedingly meagre and in bad condition, and could ſcarcely ſtand on her legs. She died towards the beginning of autumn; but rather in conſequence of the cold, and dampneſs of the climate, together with change of food and the means employed to cure an eruption, which had broken out on her ſkin in conſequence of being overheated on the journey. Notwithſtanding this malady, ſhe recovered her ſtrength ſo far as to ſhow, in part, her former agility, and that ſhe poſſeſſed properties very ſuperior to common aſſes. But the dampneſs of autumn was evidently injurious to her; for when ſhe went out into the wet fields her hoofs ſoon ſplit, and at length dropped from her feet.

All the Tartar tribes conſider the wild aſs as one of the ſwifteſt of animals, and unanimouſly aſſert, that the fleeteſt of their horſes are unable to keep pace with this light and ſlender animal. We are told by Xenophon that the wild aſſes of Meſopotamia, when hunted, ſtopped ſometimes as if to take a nearer view of their purſuers, and then ſprang forward again with ſuch velocity, that the beſt horſes could not overtake them. All the antient authors extol their ſpeed in running; and their Hebrew name, *paræd*, is derived from this property. As they are fond of feeding on the cold rocky hills and mountains, nature has endowed them with the power of going with the greateſt velocity over the rougheſt ground and along the narroweſt paths; and this property has even remained with the common ſluggiſh aſs, and from it has been tranſmitted to the mule. The ſhape of the animal ſeems as if deſtined for this purpoſe; the body being very ſlender, while the feet ſtand near to each other, and the ſmall round hoofs, which are exceedingly hard, are ſharp on the edges.

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[*] Ælian. Hiſt. Animal. lib. iv. c. 52.

[†] Le Bruyn Reize over Moſkovie door Perſien en Indien, p. 405: Adanſon Voyage au Senegal, p. 118.

[‡] Niebuhr eſtimates the diſtance which a riding aſs can travel with an uniform pace in half an hour to be 1750 paces—double thoſe of a man; whereas the largeſt camels can travel only 975, and the dromedary 1500 at moſt. See his *Reize in Arabien*, p. 311, 312.

As the wild aſs frequents the dry ſouthern diſtricts, and never wanders ſo far north as the wild horſe, herds of which are found under the 56th degree of latitude, the tame aſs does not thrive ſo well in the damp northern diſtricts as the horſe; and this, together with want of care, exceſſive labour, and bad nouriſhment, may have occaſioned the degeneration of the common aſs, and brought this breed into diſcredit. But who judge of the noble breed of our beſt horſes from the wretched animals in the poorer diſtricts, which are ſcarcely ſo good as aſſes? With proper care and management, might not as good riding aſſes be bred in the temperate and ſouthern parts of Europe, as in the Eaſt? and might not the breed of this animal be gradually improved as we have improved the horſe, the wild breed of which are far inferior in ſize and beauty? In regard to the viciouſneſs of the aſs, and particularly its obſtinacy, which is inherited by the mule, it appears to me very probable that this is owing to the too wide conformation and ſenſibility of the organs of hearing which nature has beſtowed on the wild race, which, being deſtined to live in extenſive deſerts, have need of being able to diſcover danger at a greater diſtance. The noiſe which the aſs muſt hear in the neighbourhood of men evidently ſtuns it; and, on that account, it is cuſtomary in England to crop the ears of the aſſes employed at mills, becauſe it is known that by theſe means they become much more tractable and obedient. Means might eaſily be diſcovered of weakening their hearing, and accompliſhing the ſame end, without ſuch a mutilating operation.

By improving the breed of aſſes, which, after the example of the antients, might be done ſooneſt, and in the moſt complete manner, by introducing riding aſſes from the Levant, or colts of the wild breed, the breed of mules, ſo uſeful for carrying burdens, would alſo be much improved. Varro and other antient authors, who treat of agriculture and the breeding of cattle, unanimouſly aſſert that, in their time, the beſt mules were produced from a mare and the onager, or wild aſs. The Perſian mules, the ſtrength and courage of which are extolled by Le Bruyn *, are in all probability produced in the ſame manner. Theſe qualities, perhaps, might be obtained in a higher degree if the *dſhiggetai*, or great Mogul wild aſs, which exceeds the onager in ſize and beauty †, and perhaps alſo in fleetneſs, could be once tamed, and employed for this purpoſe.

The wild aſs, in its habits and mode of life, approaches near to the *dſhiggetai* and wild horſe. They rove about in herds, which conſiſt of young colts of both ſexes, under the protection of a ſtallion. The ſame account is given by Oppian and Pliny, and by thoſe who have copied them. But it appears that at the periods of their migration the ſtallion lays aſide his jealouſy, and ſeveral herds then unite together. The ſeaſon of covering is then paſt, and the females are big with young; yet the ſtallions, even then, are accuſtomed to bite and kick each other very much.

Their hearing and ſight are ſo acute, and the weather is ſo fine, that in the open fields it is not poſſible to approach them. The Kirgiſians, therefore, endeavour to conceal themſelves in the diſtricts through which they paſs, or in the neighbourhood of the ſalt lakes, which they frequent; but the *kulans* ſeldom drink, and often not in the courſe of two days. The aſs which I had in my poſſeſſion frequently abſtained from drinking for two days, and in particular when a heavy dew, or light ſhower of rain, had fallen. She was fonder of ſaline than of freſh water; but ſhe would never touch any that was mixed with bran, or that was turbid. Bread rubbed over with ſalt ſeemed very grateful to her; and ſhe would often eat whole handfuls of ſalt. Mr. Hablizl ſaid, that while he kept her at Derbent, ſhe uſed always to run to the Caſpian ſea to drink, though ſhe could have found freſh water much nearer. Plants which contained ſaline particles, ſuch as the different kinds of kali, or the ſoda plant, gooſe-foot, and plantain, were moſt agreeable to her; and next to theſe, bitter milky plants, ſuch as dandelion, the ſow-thiſtle, and the like; and after theſe, the different kinds of clover, lucern, all leguminous plants, eſpecially when given her with the grain, and dog's graſs. She was fond alſo of green cucumbers; and ſome vegetables, ſuch as different kinds of pulſe, which ſhe did not reliſh when green, ſhe ate readily when dried. On the other hand, ſhe had an averſion to all ſweet-ſmelling balſamic plants which are eaten by the tame aſs, ſuch as mudwort, ranunculus, nettles, and all hard and prickly plants, and even thiſtles. In Perſia, the young *kulan* colts, when caught, are firſt tamed by feeding them with rice, oats, wheat ſtraw, and bread. The wild aſs eats with great avidity a plant very common in the ſouthern part of Tartary, and to which the Bucharians give the name of *bürogan*.

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* Le Bruyn ſays in his Travels above quoted, p. 139, that in the ſouthern parts of Perſia there are mules which, from natural inſtinct, like ſome ſpirited horſes, and the ſtallions among the wild horſes, attack bears and other ravenous animals with great courage, and knock them down with their fore-feet. The unfortunate ſwine, which in theſe diſtricts are black, and covered with very rough briſtles, fall ſometimes a ſacrifice to this impulſe of the mules, which are not always able to diſtinguiſh them from dangerous animals. A circumſtance of a ſimilar kind is mentioned by Varro, lib. ii. c. 8:—"I once ſaw the horſe of a Calmuc Tartar, while the rider was on his back, ruſh into a field and kill a breeding buſtard, which he, no doubt, took for ſome ſavage animal." This kind of inſtinct ſeems to be common alſo to the wild aſſes, which, according to the account given by the Kirgiſians, attack wild animals in a body. When one of them obſerves a ſnake, it immediately gives the ſignal by a loud cry; upon which all the reſt aſſemble, and each of them tries which ſhall firſt deſtroy the dangerous reptile. They do the ſame in regard to ſuch ravenous animals as they are able to overcome; but the tiger is too much for them.

† For a deſcription of this animal ſee Philoſophical Magazine, vol. ii.

The aſs I here ſpeak of was exceedingly tame, and, like a dog, followed without any compulſion thoſe who fed her and gave her drink. She could be made go any where merely by the ſmell of bread; but if any one attempted to lead her by a halter, contrary to her inclination, ſhe ſhowed as much obſtinacy as a mule. She would not ſuffer any one to ſtand behind her; and when touched with the hand or a ſtick on the back or thighs ſhe threw out her heels, with a loud ſnorting, like that emitted by a ſtallion when he kicks.

The male which died on the journey from Perſia was much larger. According to Mr. Hablizl's meaſurement, his length from the ears to the rump was 4 feet $10\frac{1}{2}$ inches; the height before, 4 feet 2 inches 8 lines; behind, 4 feet 6 inches 6 lines; the length of the head 2 feet; of the ears, $11\frac{1}{2}$ inches; and the tail, with the buſh at the extremity, 2 feet $1\frac{1}{2}$ inch. The length of the female, however, was only 3 feet 10 inches; the height before, 3 feet 4 inches 8 lines; behind, 3 feet 6 inches; length of the head, 1 foot 6 inches; the tail, $10\frac{1}{2}$ inches; the buſh at the end of the tail, 8 inches 5 lines; the ears, 7 inches 5 lines; and ſhe weighed, when the bowels were taken out, only about 165 pounds apothecaries weight. The male was alſo much more ſtrongly made in the neck, limbs, breaſt, and rump; and was diſtinguiſhed from the female by a ſmall ſtripe over the ſhoulders croſſing that on the back, which was wanting in the female, the latter having only the ſtripe on the back. This croſs is much more common among the tame aſſes, and is obſerved, in particular, on thoſe which are of a bright colour. The Kirgiſians ſay, that among the wild aſſes the croſs ſtripe is found ſometimes double.

The onager is much taller and finer limbed than the common aſs. The female in my poſſeſſion was ſo narrow in the cheſt and rump, that behind ſhe had the appearance of a young filly, as may be ſeen in the plate. Like young fillies, alſo, ſhe could ſcratch her head and neck with the hind foot, which a full grown horſe cannot do. On the fore-legs ſhe appeared very weak; but ſhe could carry on her back the heavieſt man, and even run with him. She always held her head erect, in a more graceful manner than the common aſs; kept her ears always upright, even during illneſs; and in all her motions ſhowed great agility.

The head of the onager is higher and larger than that of the *dſhiggetai*, and yet I found the ſkull, when cleaned, to be remarkably light. The head is much bent, like that of a ram: the fore-part between the eyes is flat; but above the eye-ſockets, which are as ſtrong as thoſe of old horſes, it is elevated in a flattiſh round form. The lips are very thick, and to the very edges cloſely covered with ſtiff hair like briſtles, which towards the round part of the mouth lie crooked. The noſtrils do not form that excreſcence-like elevation which is peculiar to the *dſhiggetai*. The pupil of the eye is yellowiſh brown. The ears at the points are quite black, covered on the inſide with curled hairs that croſs each other, which grow partly on both edges of the ear, and partly on three ridges which run along the hollow of the ear lengthwiſe.

The colour on the muzzle and the greater part of the body is a beautiful white, with a kind of ſilvery appearance; but the upper part of the head, the ſides of the neck, and the rump, are of a pale iſabella colour. This colour does not extend along the fore-thighs: it however covers the haunches; although on the ſides of the belly a white ſpace, of about a hand breadth, ſeparates the colour of the belly from that of the thighs: a white band, alſo, runs along each ſide of the ſtripe on the back, and unites itſelf to the white part of the lower belly. Oppian has expreſſed very accurately this white ſeparation, in his poetical deſcription of the onager. The mane, which is blackiſh brown, begins between the ears and extends to the ſhoulders; it conſiſts of ſoft woolly hair from three to four inches in length, and ſtands erect exactly as in newly-foaled horſes. The ſtripe on the back begins at the mane, and ends at the commencement of the tail: it is almoſt of a coffee-brown colour, broader towards the *os ſacrum*, pointed towards the tail, and every where covered, even in ſummer, when the animal is almoſt perfectly ſmooth, with thick matted hair, which ſeems very different from the reſt. The hair which forms the buſh at the end of the tail is almoſt as ſtrong as that of the mane of the horſe, and a full ſpan in length.

The ſort of ſcar in the inner ſide of the fore-feet, which in the common aſs is round, I found in the wild aſs to be longiſh, but not perfectly oval. The hoofs of all the four feet have in the place of the ſpur an elevation covered with a horny ſkin. The hoofs are almoſt round, and have deep cavities on the ſoles.

The hair of the onager, and particularly in the winter, is much ſilkier and ſofter than that of the horſe: it may be compared to the beſt camel's hair. The winter wool is matted, has an oily touch, and where the iſabella colour prevails, that colour is much paler, a few places excepted, where it is ſhaded in a different direction, and particularly according to the lines expreſſed in the figure.

In the onager I counted only ſixteen articulations of the tail; the others were in number equal to thoſe of the common aſs. It had only thirty-two teeth, viz. in each jaw ſix grinders very much uſed, and in each ſide row five cheek-teeth. In the interior parts I could obſerve no further difference between them and thoſe of the common aſs than are often to be found in animals of the ſame ſpecies. In the great gut I found very large worms (*aſcaris pollicaris*), ſuch as are frequently found in horſes; and here and there in the ſmall guts ſome aſcarides, ſmaller than thoſe common in man. Thoſe writers alſo who aſſert that the aſs is never infeſted by external vermin, muſt not extend this obſervation to the wild aſs; for the one here deſcribed was ſo covered with a ſort of ſmall lice, that the hair ſeemed as if ſtrewed over with them.

I have already ſaid that the Kirgiſians prefer the fleſh of the *kulan* to all other game, and even to that of their young horſes, of which they are ſo fond. The ſame taſte ſeems to prevail among the Arabs [*]; and the writers of that nation, in regard to this food, make the ſame diſtinction between the wild and tame aſs as the Hebrews made between both animals;

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[*] See Bochart's *Hierozoicon*, lib.iii. c. 16; and Forſkal's *Obſervat. Zoolog.* p. 5, where the onager is mentioned under the Arabian name *djaar*.

mals; and they conſider it unlawful to mix the onager with the tame aſs, as they do all mixed breeds of other animals.

The ſkin of the wild aſs is highly valued by the Bucharians for making ſhagreen. Rauwolf ſays the ſame thing of the Syrian wild aſs, the ſkins of which are brought to Tripoli for ſale. Thoſe, however, who have hitherto believed that the ſkin of the wild aſs is grained of itſelf, and like a natural kind of ſhagreen, or that ſhagreen can be made of no other ſkin than that of the wild aſs, are much miſtaken; yet this erroneous opinion is to be found in ſome modern writers, and has been adopted even by Buffon. As I had an opportunity at Aſtracan of ſeeing the proceſs uſed for the preparation of the fine ſhagreen, in order to deſtroy this falſe idea I gave a deſcription of it in a former volume of this work [*]. The Bucharians, however, convert whole ſkins of the wild aſs into a kind of coarſe black ſhagreen, of which they make boots of a ſingular form, but remarkably durable, and having the ſoles entirely covered with nails: theſe boots are uſed alſo by the Kirgiſians, and purchaſed at a high price: the fine ſhagreen, on the other hand, is made chiefly in Perſia, and from a particular part of the hides of horſes. Reſpecting the ſtones found in the wild aſſes, and of which Bauhin, in his Latin Treatiſe on the Bezoar, diſtinguiſhes two kinds, I can ſay nothing: they are the ſtones, perhaps, found in the common aſs or mules, which, in order to give them greater importance, are pretended to be thoſe of the above animal, in the ſame manner as ſtones cut from horſes are brought from Perſia and India under the name of Garmandel or Coromandel bezoar. I ſhall paſs over in ſilence all the fables related of the onager which may be found in Geſner, Aldrovandi, and Bochart. The miſtake of ſome modern writers, who conſider the onager of the antients as the zebra, or who have confounded it with the *dſhiggetai*, is unworthy of refutation. To be fully convinced that the wild aſs here deſcribed is beyond all doubt the onager of the antients, nothing will be neceſſary but to compare this deſcription with thoſe of Oppian and other writers; and with the paſſage of Luitprand, biſhop of Cremona, quoted by Bochart.

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[*] For this deſcription ſee Philoſophical Magazine, vol. vi.

XIII. *Account of ſome Improvements on Water-Wheels.* By *Mr.* ROBERTSON BUCHANAN, *Engineer, Glaſgow.*

To Mr. Tilloch.

DEAR SIR, Glaſgow, Oct. 20, 1801.

A CORN MILL in which I had an intereſt having had about two feet of its fall unoccupied, owing to the overſhot wheel having been too ſmall in diameter, I was deſirous to apply this unoccupied part of the fall in ſome way to increaſe the power of the mill. Thinking over various expedients for this purpoſe, in February 1798 an idea of a water-wheel occurred to me, which I thought might be applied with great advantage not only to the ſituation I had in view, but in all other caſes of low falls. Some circumſtance, however, prevented me at the time from putting it in practice; but I had afterwards, at different times, two models of it made; and in November 1799 ſent up the accompanying deſcription of it to my friend Mr. Mundell, of Fludyer-ſtreet, requeſting him to give it to the public. Upon ſome after reflection, however, I conceived it beſt, for ſome time, to delay its publication; and accordingly till now have allowed it to lie dormant.

Soon after finiſhing the drawing referred to in the deſcription, I thought the particular form of buckets there repreſented might be a little altered, ſo as to make them ſtill more advantageouſly contain the water. Upon ſhowing the alteration I propoſed to my ingenious friend Mr. JOHN ROBERTON of this place, he was at conſiderable pains to inveſtigate the ſubject, and pointed out the form of buckets repreſented in fig. 3.

Allow me further to direct your attention to the conſtruction of the arms ſhown in the figures. It is applicable to any kind of water-wheel; and ſince 1790, when I firſt conſtructed a wheel with arms on that principle, a conſiderable number of large wheels have, in Scotland, been erected on that plan. I ſhall annex a ſhort deſcription of this conſtruction of arms.

I remain,
Dear Sir, yours,
ROBERTSON BUCHANAN.

Copy of the Deſcription ſent to Mr. Mundell.

It has been aſcertained, from the moſt accurate experiments on water-wheels, that under the ſame circumſtances an overſhot produces about double the effect of an underſhot wheel.

Hence it is obvious, that where water can be made to act on a wheel by weight, it is much more effectual than when the ſame water is made to act by impulſe.

But where the fall is leſs than about half the diameter of a wheel with buckets made in the uſual form, the difficulty of filling them, and the ſhort time they are able to retain the water, are ſuch great defects, that in ſuch caſes wheels with open float boards, on which the water acts partly or altogether by its impulſe, have been found in practice to be more advantageous.

It occurred to me however, that, by adopting another form of the buckets, they might be ſo made as to be eaſily filled, and at the ſame time capable of retaining the water in a ſituation to produce nearly its full effect, altogether by weight, on a low fall.

A wheel of the conſtruction I mean is repreſented in the figures. Contrary to the uſual practice, the water muſt here be poured into the buckets *from within* the ſhrouding*.

How the filling of the buckets from within can be accompliſhed, may not at firſt be obvious; but a ſlight inſpection of the figure will ſhow that it may be done without the penſtock interfering with the arms of the wheel.

The buckets in the figure empty themſelves by means of apertures the whole length of the buckets, but no wider than juſt ſufficient to diſcharge the water before they begin to aſcend.

References to the Figures.

Fig. 1. (Plate III.) elevation of the water wheel.

A, A, the penſtock.

From B to C a part of the wheel is repreſented with the ſhrouding removed, to ſhow the form of the buckets and the ſituation of the water in them.

a a a the apertures by which the water eſcapes from the buckets.

Fig. 2. horizontal ſection of the wheel, and plan of the penſtock.

b the aperture by which the water enters from the penſtock to the buckets.

ROBERTSON BUCHANAN.

Rotheſay, Nov. 9, 1799.

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* The ſhrouding is a flat ring, generally of wood, of the diameter of the wheel, ſerving to form the ends of the buckets, or to connect the floats, expreſſed by the letter B, (Plate III.)

Deſcription of the Conſtruction of the Arms.

Fig. 1. and 2. It is evident that arms, as commonly fixed in mortiſed ſhafts, are weakeſt in one direction, and that commonly the direction of the ſtrain. To remedy this defect, the *feather-pieces* FF are applied all round, having their broadeſt ends towards the centre of the wheel, and being at right angles to the breadth of the principal arms. In order to unite them firmly to the principal arms, and connect the whole more firmly, a ring of iron, R, is applied on each ſide: blocks of wood, W, put in the vacant ſpace between, and the keys KK, bring the whole home to the ſhaft.

N. B. In ſome caſes it may be eaſieſt to introduce the water in the direction XY, as it was in one of the models, inſtead of that on the ſhaded part of the plan.

XIV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES AT BERLIN.

On the 6th of June the academy held a public ſitting to celebrate the anniverſary of the king's birth-day.

Prize Queſtions.

The Phyſical Claſs had propoſed the following queſtion for the year 1801:

"Has electricity any influence on matters which ferment? If it has, what is its action? Is it favourable to fermentation, or does it prevent it? Does it produce changes in the products of fermentation? What advantages may be derived from the development of this matter to improve the art of making wine; and thoſe of brewing, making vinegar, and diſtilling ſpirits?"

But as it received only one memoir on the ſubject, which was not ſatisfactory, the claſs propoſes the ſame queſtion for the year 1803, with this expreſs condition, that "thoſe who attempt to anſwer it, will do ſo by a ſeries of eſſays and experiments calculated to produce evident and certain reſults."

The Philoſophical Claſs propoſed for the year 1803:

"Can a moral appreciation of actions be taken into conſideration in the eſtabliſhment and application of penal laws? and, if it can, to what extent?"

The Claſs of the Belles Lettres again propoſed the following queſtion for 1803:

On the Goths and Gothiciſm.

"1. Among the nations who contributed to haſten the downfall of the Roman empire, did the Goths diſtinguiſh themſelves above others by any peculiar qualities in regard to their conſtitution, their laws, cuſtoms, and uſages; or, more particularly, in regard to their literature and arts?

"2. Are not the terms *Gothic* and *Gothiciſm* denominations created at poſterior periods to denote the ſtate in which the ſciences, letters and arts were, after the decline of the Roman empire, and during the middle ages?

"3. If this be the caſe, at what period did theſe terms begin to be uſed in this more general acceptation?"

The Mathematical Claſs has alſo repeated, for 1802, the following queſtion, with a double prize:

"As there ſtill remain, notwithſtanding the labours of the ableſt aſtronomers, ſeveral points to be cleared up in regard to the variation of the obliquity of the ecliptic, the academy invites the learned to employ themſelves again on this object, and will adjudge the prize to that memoir which ſhall contain the moſt intereſting reſearches and the moſt important illuſtrations of this point.

"It invites the learned of all countries, except the ordinary members of the academy, to turn their attention to theſe queſtions. The prize, which conſiſts of a gold medal of the weight of fifty ducats, will be adjudged to thoſe who, in the opinion of the academy, ſhall have given the beſt anſwer. The papers, written in a legible hand, muſt be addreſſed (poſt paid) to the perpetual ſecretary of the academy. Thoſe which, in the opinion of the academy, have not ſufficient merit to be entitled to the prize, cannot be returned to the authors, as the originals muſt be depoſited among the archives of the academy: all thoſe, therefore, who tranſmit memoirs as candidates for the prize, are requeſted to take copies of them before they tranſmit them to the academy.

"The term for receiving the memoirs is fixed at the 1ſt of May of the above years, after which period none can be received, whatever reaſon may be aſſigned for the delay."

FRENCH NATIONAL INSTITUTE.

In the public ſitting of the Inſtitute on the 7th of October, the following papers were read:

An account of the obſervations made on the perpendicular chains (plumb lines) ſuſpended from the dome of the Pantheon, the object of which is to aſcertain whether any warping has taken place in that edifice: by C. Prony;

On that kind of pavement called by the Greeks *lithoſtrata*, and by the moderns *moſaic:* by C. Gibelin;

An hiſtorical account of the life and ſervices of H. F. Gilbert: by C. Cuvier;

On public ſpirit: by Toulongeon.

The following notice of the labours of the Claſs of Literature and the Fine Arts was read by C. Villars:

Antiquities.

The names of two celebrated cities, Herculaneum[*] and Pompeii[†], have often inflamed with a noble emulation the amateurs of antiquities and antient monuments. Hitherto the total diſappearance of theſe two cities has been aſcribed to the famous eruption of Veſuvius in the firſt year of the reign of Titus, and the 79th of the Chriſtian æra. According to the obſervations of C. Du Theil, this epoch is not certain; for theſe two cities roſe again from their ruins under the reign of that prince; and they ſtill exiſted, with ſome remains of ſplendour, under Hadrian. The beautiful characters of the inſcription traced out on the baſe of the equeſtrian ſtatue of M. Nonius Balbus, ſon of Marcus, are an evident proof of its exiſtence at that period. They were found under the reign of the Antonines; for the account of Trimalchio's feaſt, in the ſatirical romance aſcribed to Petronius Arbiter, furniſhes ſeveral evidences of the exiſtence of Pompeii, and of ſome of the edifices of Herculaneum, under the laſt of theſe princes. In the geographical monument known under the name of Peutinger's chart, which is of a date poſterior to the reign of Conſtantine, that is to ſay, in the commencement of the fourth century, Herculaneum and Pompeii were ſtill ſtanding, and then inhabited; but in the Itinerary improperly aſcribed to Antoninus, neither of theſe two cities is noticed; from which it may be conjectured, with ſome degree of foundation, that the entire ruin of Herculaneum and Pompeii muſt have taken place in that interval, between the time when Peutinger's chart was conſtructed and that when the above Itinerary was compoſed.

The eruption which took place in 471 occaſioned the moſt dreadful ravages. If we can give credit to Marcellinus, the aſhes thrown up by Veſuvius covered all Europe. They were carried as far as Conſtantinople; where, according to our chronicler, an anniverſary feſtival was inſtituted in remembrance of this ſtrange phænomenon. It is very probable that the cities of Herculaneum and Pompeii diſappeared at that period, and that no more traces of them were left. This conjecture is ſupported by a letter written by Caſſiodorus in the name of Theodoric, who reigned between 493 and 526. On this authority C. Du Theil is inclined to think that, after the fatal eruption of 471, ſuch of the inhabitants of Pompeii as had the good fortune to eſcape alive, retired to Nola in Campania, and that thoſe who eſcaped from Herculaneum took ſhelter at Naples. The quarter where the latter eſtabliſhed themſelves was detached from the other parts of the city.

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[*] Herculanum, otherwiſe Herculanium, Herculaneum, Herculeum, an antient city of Italy in Campania, on the ſea coaſt, oppoſite to Veſuvius. *Plin.* lib. iii. c. 5. places it between Naples and Pompeii. *Paterculus*, lib. ii. c. 6. as well as *Florus*, lib. i. c. 6. ſays that it was conquered by the Romans during the war of the allies.

[†] Pompeii, in Campania, four miles from Naples towards the eaſt. Under the aſhes of Veſuvius was diſcovered the village called *Torre del Greco*—the Greek tower. It is generally ſuppoſed that the city of Pompeii was buried in that ſpot.

This may ſerve to explain the denomination *Regio Herculanenſium*, or the quarter of the Herculaneans, which has been obſerved on ſeveral monuments of ſtone dug up at Naples, and alſo in ſeveral inſcriptions, which have been collected and publiſhed by learned Neapolitan antiquaries.

After Du Theil had read this memoir, he communicated a notice reſpecting ſome objects of antiquity lately found near the ſmall town of Azai-le-Nideau, on the left bank of the Indre. The account of them was tranſmitted to him by C. Biencourt. Some workmen, when digging a ditch, diſcovered a coffin containing a young child, as was concluded from its teeth, cranium, and ribs; but the reſt had ſuffered ſo much from the injuries of time, that it fell into duſt when expoſed to the air. At the ſide of this body were the following objects:—1. Two poniards, one of iron, the other of ivory, half deſtroyed: 2. A gold bulla ſhaped like an acorn—a badge worn by the children of Roman patrician families: 3. A ſmall jaſper ring ſet in gold, on which are engraven two veiled figures: 4. A ring entirely of rock cryſtal. It has engraven on it two rams yoked to a car: a ſmall Cupid ſitting in the car urges them on full ſpeed. This engraving diſplays much grace and ſpirit. 5. A cryſtal ring imitating a row of pearls ſurmounted with a ſhell: 6. A priſm of twenty planes, of rock cryſtal, very regularly cut: 7. Four lachrymatories of glaſs: 8. Several figures ſculptured in ivory, and almoſt decompoſed: 9. A young Cupid alſo of ivory, pretty well pulverized. In the ſame tomb were found ſeveral works compoſed of reſinous ſubſtances, which time ſeems to have reſpected. 1. A lion holding under his paws a dog, the head

of

of which he is devouring: 2. Another piece of ſculpture repreſenting a car drawn by an old goat. A Cupid ſitting in the car makes a ſign to the goat to advance, and threatens him with a whip which he holds in his hand. Before the car a young woman is ſeated without veſtments, and behind it is an old man clothed in a long robe. This piece ſhows a great deal of expreſſion. 3. A ſilver cup, the bottom of which is oxidated: rings and broaches of metal, on which are ſiled reſinous perfumes. The deſcription of theſe articles, which C. Biencourt will ſoon tranſmit to the Inſtitute, muſt be highly intereſting to artiſts and antiquaries.

C. Mongez, always employed in reſearches reſpecting the coſtume of the antients, has been endeavouring to diſcover and deſcribe the different ſubſtances which they employed for their veſtments, arms, &c. Particular reaſons have induced him to detach from his work an eſſay on the uſe which the antients made of hemp. Heſiod and Homer do not mention this vegetable. Herodotus ſays that it reſembles flax, from which it differs only in ſize and in height. The appearance of theſe two plants, however, is marked with ſuch a ſtriking difference, that the father of hiſtory, ſince he ſpeaks ſo incorrectly, muſt have never ſeen hemp. He nevertheleſs tells us, that the Thracians procured from the northern countries of Europe the hemp which they employed to make cloth for their veſtments.

Ariſtophanes ſpeaks of a hemp merchant; and we know that the Greeks uſed hemp for cordage, and for caulking their veſſels. But they did not then cultivate this production, for it is not mentioned by Theophraſtus; and Hiero procured hemp from the northern countries of Europe; that is to ſay, from the banks of the Rhodanus, which throws itſelf into the Viſtula, and which he calls, improperly, the Eridanus. It is ſtill from Ruſſia and Livonia that Europe is ſupplied with hemp: it is very probable that it was not cultivated by the Greeks till about the commencement of the Chriſtian æra.

The Romans, like the Greeks, employed this ſubſtance for cordage, and for caulking their veſſels; but neither of them manufactured ſail-cloth of it, if we are to judge from the Greek and Roman writers whoſe works have been preſerved. C. Mongez has been obliged to deſcend from Herodotus, in his paſſage reſpecting the Thracians, to the 12th century, to find a paſſage which makes mention of hempen cloth. It is ſtill in a northern country, viz. England, that he found it.

In the 13th century, and the following ones, hempen cloth became very common in the middle and ſouthern parts of Europe. There can be no doubt that the remains of this cloth, employed with thoſe of linen for making paper, then introduced into Europe, contributed to preſerve the antient authors whoſe works were effaced; that the ſame parchment might be employed for the books of the church and treatiſes of theology. Mongez concludes his reſearches by comparing the paſſages in antient authors, where mention is made of torrefied hemp-ſeed, with the ſmoke of which barbarous nations got intoxicated, and the practice ſtill common in Egypt and Aſia of chewing theſe ſeeds for the like purpoſe.

The Claſs of the Mathematical and Phyſical Sciences had propoſed in the year 8, as the ſubject of a prize, to be adjudged on the 7th of October 1801, the following queſtion:

"To point out the earthy ſubſtances, and the proceſſes proper for making earthen-ware capable of ſtanding a ſudden tranſition from heat to cold, and which may be within the reach of all claſſes of people."

This queſtion was accompanied with the following notice: —The art of manufacturing that valuable pottery known under the name of *porcelain* has been brought to a degree of perfection in France which leaves nothing more to be deſired; but the caſe is not the ſame with the common pottery employed for daily uſe: the latter is far from being carried to that degree of increaſe and proſperity ſo neceſſary for the wants of the great body of the people; while ſome neighbouring nations, who do not make ſo fine porcelain, manufacture very uſeful pottery, the properties of which are far ſuperior to that made in France. The Inſtitute requires that the competitors will examine the compoſition of theſe excellent kinds of pottery, the earths which may be employed in the compoſition of them, and the artificial mixtures which might be ſubſtituted in their ſtead: the manner in which theſe earths ought to be treated to give them the neceſſary qualities; the art of baking them; the degree of heat; the form of the furnaces moſt advantageous; and, in particular, the proceſſes for glazing pottery without oxides and pernicious metals.

The claſs received only one memoir on this ſubject, accompanied with ſeveral ſpecimens of pottery, and of the earths of which they were made. It found that the memoir was written in a clear and methodical manner; that it contained principles acknowledged by philoſophy and chemiſtry; and that the details, ſerving to illuſtrate them, announced long practice in the art of pottery, together with great theoretical knowledge: but as the ſpecimens which accompanied this memoir did not anſwer the object pointed out by the claſs, it has prolonged the term for adjudging the prize to the 3d of October 1802, ſtrongly recommending to the candidates to apply with particular care to this branch of manufactory.

This double prize, of the value of 6800 francs (above 280*l.* ſterling), will be adjudged in the ſitting of the Inſtitute at the above period.

The Claſs of Literature and the Belles Lettres propoſed in the year 8, as the ſubject of a prize in antiquities, the following queſtion:

"What are the ſtudies which form, and the kinds of knowledge which characterize, the antiquary? What advantages ought to ariſe to ſociety from ſuch knowledge?"

On account of the importance of this ſubject, the claſs has thought proper to prolong the period of receiving memoirs till the 3d of October 1802, as before.

The prize will be a gold medal of the value of five hectogrammes, and will be adjudged in the public ſitting of January 5, 1803.

XIV. *Intelligence and Miſcellaneous Articles.*

October 1801.

MEDICINE.

THE *Oberdeutſche Allgemeine Literatur-Zeitung*, July 23, 1801, under the head "Literary Notices," contains the following article:

"In the 7th number for the year 1779, of that generally eſteemed journal *Der Rathgeber für alle Stande*, the queſtion, Whether friction with oil can be employed for preventing and healing the ſmall-pox, having been anſwered in the affirmative, both by the Rev. Charles Gottfried Bauer, of Frohburg, and the learned editor Dr. Daniel Collembach; and as they produce ſeveral important grounds to prove their aſſertion, an individual who has the good of mankind at heart wiſhes to ſee it confirmed by facts. For this purpoſe, he offers a prize of twelve ducats to the phyſician who, before the end of the preſent year, ſhall have ſucceſsfully employed friction with oil on the greateſt number of variolous patients. The ſmall-pox, however, muſt have been of the malignant confluent kind, and accompanied with ſymptoms conſidered as fatal.

"Any phyſician alſo, who ſhall prove, by the greateſt number of facts, that he has preſerved whole families, by friction with oil, from the infection of this diſeaſe, will receive a like prize of twelve ducats. We therefore expect to be made better acquainted with the rules for employing this remedy ſo much extolled, and which Dr. Fothergill in England, and other eminent phyſicians, found uſeful in putrid fevers."

MINERALOGY.

M. Chenevix has had under examination a ſpecimen of *manachanite* from Botany Bay. It differs from the Corniſh in containing a white quartzy ſand. It yielded on analyſis,

Silex - -	11
Oxide of iron -	49
Oxide of titanium -	40
	100

It is difficult to obtain this metal reduced and pure; but by mixing the oxide of titanium with glaſs of phoſphorus and charcoal, and then employing a flux of borax, a button of phoſphuret of titanium may be obtained.

If the mineralogical riches of New Holland may be judged of from the valuable productions with which we have become acquainted already, we may look forward to many intereſting diſcoveries: already a metal is found to be a native of thoſe climates which has not long been known, and has been but rarely met with in Europe.

D'Andrade, in his travels through Sweden and Norway, has added to the catalogue of minerals the following ſubſtances, of which he intends publiſhing a particular account: Acanthicone, ſpodumene, ſahlite, ichthyophthalme, coccolite, aphrizite, allochroite, indicolite, wernerite, petalite, chryſolite, ſeapolite.

Klaproth has completed the analyſes of the fluated alumine called the *chryſolite of Greenland*. It contains

23·5	of alumine
36·0	of ſoda
40·5	of fluoric acid and water.
100·0	

He conſiders the diſcovery of ſoda in ſtones as very remarkable. It is the firſt inſtance of ſuch a quantity of ſoda being preſent in an inſoluble ſubſtance.

Vauquelin has repeated the analyſes: he finds

Alumine - - -	21
Soda - - -	32
Fluoric acid and water -	47
	100

Upon a careful examination of the alumine he thinks it contains 5 of ſilex.

Klaproth has alſo diſcovered a native phoſphate of copper; and is at preſent employed in analyſing this new combination of the phoſphoric acid.

CHEMICAL NOTICES.

Some experiments made by Mr. Carliſle prove that ſulphurated hydrogen gas is not evolved from eggs unleſs they are heated in water, and this by the decompoſition of the water uſed.

A perſon in the colour line, on the continent, having mixed coarſe Pruſſian blue and nut oil for a blue colour, covered them with water, and left them till wanted. On going to uſe it, he found it changed to white: thinking it required more Pruſſian blue, he ſpread it upon a ſtone, when the colour came again of itſelf: he then covered it with oil inſtead of water, but with no better ſucceſs. As an experiment, ſome was laid on white paper, ſome on wood, and ſome on a wall. That on the white paper was the laſt that returned to the blue colour; but they all became a ſtrong blue.

Phoſphorus expoſed in hydrogen gas is diſſolved in proportion to the temperature. If this is done in a light place, the phoſphorus will ſublime on the inſide of the glaſs. If the glaſs be darkened in parts where the light cannot enter, the phoſphorus will not have ſublimed. This adheſion to the ſides of the glaſſes never takes place but when light is preſent.

Mr. Cruickſhank has publiſhed a ſecond paper on the gaſeous oxide of carbon, in which he propoſes a method of analyſing theſe gaſes by the oxymuriatic acid gas, which abſorbs and decompoſes the hydrogen and the hydro-carbonate, leaving the gaſeous oxide. In this caſe, not more than a double quantity of the oxygenated muriatic acid gas ſhould be uſed. The gaſeous oxide itſelf may alſo be decompoſed by the oxygenated muriatic acid gas, provided a ſufficient quantity be expoſed to its action. Hydrocarbonate or hydrogen may be immediately decompoſed by the action of oxymuriatic gas and the electric ſpark; but the gaſeous oxide of carbon and oxymuriatic acid gas are not fired by the electric ſpark.

Mr. Cruickſhank gives the following as an entertaining experiment:—Fill a veſſel, of about the capacity of three pints, with oxymuriatic gas; ſee that it is drained from the water in it; then throw into it about a dram of good ether, and cover the veſſel with a piece of paper. In a few ſeconds a white vapour will be obſerved moving circularly: this will be followed by an exploſion and flame, and a large quantity of carbon will be depoſited.

GALVANISM.

Mercier has quoted a work, publiſhed 27 years ago, which contains the principle of galvaniſm. Twelve years before, the celebrated Sulzer diſcovered this principle, and obtained the ſame reſult. "If you join," ſays he, "two pieces of metal, one of lead and the other of ſilver, in ſuch a manner that their edges form one plane, and if they be placed upon the tongue, a taſte of the vitriol of iron will be felt, while each piece ſeparately leaves no trace of taſte."—*Smith's Poſthumous Works*, part ii. p. 308.

NATURAL HISTORY.

A very ſtrong leaden pipe, which conveyed water into a baſon in the garden of the palace of the Conſervative Senate, the ci-devant Luxembourg, and which has not been in exiſtence for 30 or 40 years paſt, has lately been diſcovered. This pipe contained a ſediment depoſited by the water, which, hardened by time, had become ſtone, or rather freeſtone, ſufficiently cloſe to ſtrike fire with ſteel without the lead which contained it being in any manner altered. Had the workmen taken the precaution to remove the lead without touching the matter, they would have preſerved a curioſity unique in its kind; a natural pipe from 2500 to 3000 feet long, of the moſt compact freeſtone, and an inch and a half in thickneſs. In its whole diameter there were counted about fifty concentric ſtrata or coats.

COMETS.

"In the *Journal de Paris* of May 28, Caigné the notary announced, that he would give 600 francs to the perſon who ſhould diſcover a comet. On the morning of July 12, C. Pons, keeper of the obſervatory of Marſeilles, diſcovered one, and in the evening it was obſerved alſo by three able aſtronomers at Paris. The Board of Longitude being requeſted to adjudge the prize, thought that C. Pons had a kind of priority, though he did not make his obſervations known till the evening: beſides, the three aſtronomers of Paris conſented, and C. Pons had the prize. I ſolicit the government to propoſe prizes in the ſame manner, to accelerate our knowledge of comets, the only branch of aſtronomy which has not made progreſs. It is, however, ſo eaſy to find comets, that thoſe not aſtronomers may diſcover them when they chooſe.

LALANDE."

ASTRONOMY.

"Who can decide, when doctors diſagree?"

"For ſome years the ſpots of the ſun had become rare; but on the 23d of September, in the morning, I obſerved a beautiful round and well-defined one, and found it to be 22 degrees to the north of the ſolar equator. This is a parallel in which very beautiful ones have been ſeen; which confirms me in the opinion which I advanced, (*Memoires de l'Academie* 1776 and 1778,) that there are eminences in the opake and ſolid nucleus of the ſun, where ſome ſcoriæ, which float on this ocean of fire, are ſtopped from time to time. This does not agree with the idea of Dr. Herſchel; but this celebrated obſerver is more to be admired for his obſervations than his hypotheſes. LALANDE."

CULTURE OF POTATOES.

Extract of a Letter from Sir John Methuen Poore, Bart. to Sir William Pulteney, Bart.

SIR, Ruſhall, April 4, 1801.

I can prove, not by theory, but practice, the benefit of planting potatoes on fallows. In the pariſh where I reſide, the whole of which, except five acres, is my property, there are thirty cottages, containing 131 poor people. I have, for five or ſix years paſt, allotted, free from rent, four acres of land, intended to be ſown with wheat the following autumn, for the cottagers to plant with potatoes; by which means each raiſes from ten to fifteen ſacks (equal to 240 pounds per ſack), yearly, in proportion to the number of their children: each has not only ſufficient for his family, but he is enabled alſo to fat a pig. They declare, was I to give among them a hundred pounds, it would not be of ſo much benefit to them; and it is not one ſhilling out of my pocket, for I have as good, if not a better crop of wheat from this land, as I have from the other part of the field.

The method I take is this:—The latter end of November I plough the land; the froſt during the winter mellows it: the beginning of March following, I plough it again, and harrow it; at both which times I have little to do with my horſes: I then divide it into lots; a man with a large family has a larger lot than a ſingle perſon, or one who has only two or three children, allowing about five perches (of 16½ feet ſquare) to each in a family: they then plant it, and put over their potatoes what manure they have collected the year preceding (for every cottager has more manure than neceſſary for this from their fires, and a variety of other things); and during the ſummer, after their day's labour is done, they and their wives hoe them; and as every man works more cheerfully for himſelf than for another, they do not ſuffer a weed to grow. In October they dig them up; and it is the moſt pleaſant thing imaginable, to ſee the men, their wives and children, gathering the produce of their little farms, which is to ſerve them the enſuing winter. Was this plan generally adopted, the labourers would conſume but little corn; which would ſupply the manufacturing towns, and we ſhould have no occaſion to import. As four acres are ſufficient for thirty families, it would take but a ſmall quantity of land from every farm in the kingdom. The way practiſed here, is to plant the potatoes in furrows, eighteen inches apart, and a foot apart in the rows. The land about me is of different qualities: on the hills, rather light; in the vale, near the pariſh, inclining to clay; but all fit for turnips: the potatoes are planted in the low land, being nearer home. The poor, at preſent, will not live entirely without bread, as many do in Ireland, though potatoes daily get into uſe more and more; and I am perſuaded, was my plan generally adopted, in two or three years the labourers in the country would conſume but little or no corn. Thirty years ago, the poor in this part of the country would not eat potatoes, if they could get other roots or vegetables.

SINGULAR PHÆNOMENON AT COLNEBROOK DALE.

On Sunday, the 7th of September, a moſt tremendous fall of rain, accompanied with thunder, raiſed the river Colne to ſuch a height, as to make it reach one of the blaſt-furnaces, belonging to the Colnebrook Dale company, by the overflowing of the dams. The inſtant the water entered the furnace, a dreadful exploſion took place, and a column of melted and red-hot mineral was diſcharged into the air, in a perpendicular direction, upwards of 150 feet. The exploſion was repeated at intervals for two or three times, accompanied each time by an extremely brilliant column, the heat of which was ſo intenſe, as to be felt ſeveral hundred yards diſtant. The roofs and floors of the contiguous buildings were diſplaced by the concuſſion. It is remarkable, that the prodigious quantity of matter, upwards of 2,000 cubic feet of iron-ſtone, lime-ſtone, and coals, part in fuſion, emitted from the mouth or top of the furnace, is entirely diſſipated; and that, notwithſtanding the principal part of it was large pieces of iron ore and limeſtone, yet not a particle of either can be found any where near the ſpot, nor is a veſtige of them to be diſcerned on the tops of any of the adjacent buildings. It is alſo wonderful, that the aperture at the top of the furnace (an opening not exceeding a few feet in diameter), through which the contents were exploded, is not in the leaſt rent, but is now as ſound as ever, together with the furnace itſelf.—It is ſuppoſed a water-ſpout muſt have burſt near the ſpot, as part of the works was in a few minutes completely inundated, and the water attained to the amazing height of 17 feet in a very ſhort ſpace of time.

PARTICULARS RESPECTING THE SAVAGE OF AVEYRON:

Extracted from the Report made to the National Inſtitute by Citizen Degerando.

When the young creature, known by the name of the *Savage of Aveyron*, was diſcovered in the foreſt of Canni, and brought to Paris by the profeſſor Bonaterre, the public, for a conſiderable time, echoed with this intelligence. It occupied the idle, attracted the curious, and gave riſe to a multitude of diſcuſſions, which were at leaſt premature, as they could then have no foundation but conjecture.

In the mean time, the public, who ran in crowds to ſee this child, were aſtoniſhed to behold in him only a being, nearly inſenſible, which appeared neither to hear nor to ſee, which gave no ſigns of attention, and appeared to have no ſort of active principle. Thus the intereſt which he excited became extinct as ſoon as it was raiſed.

The ſpirit of ſyſtem then paſſed a new ſentence on him. Some perſons, according to whoſe opinions the ſavage ſtate is not only the primitive, but alſo the moſt perfect ſtate of man, were ſurpriſed that this child of nature correſponded ſo ill with their ideas; and, dreading that he would completely overturn their hypotheſis, they haſtened to ſecure themſelves from any concluſion that might be deduced from him by declaring that he had been born a fool.

Superficial minds confirmed this ſentence. The ſavage of Aveyron reſembled a fool; it was therefore eaſy to conclude that he was a fool; and, what lent a ſtrong confirmation to this opinion was, that Pinel, a phyſician, who has acquired a high reputation by his ſucceſſes on perſons of diſordered intellects, having accurately examined all the circumſtances connected with the phyſical and moral ſtate of this boy, and having compared them with thoſe of the idiots confined at the hoſpital of the Salpetriere, found ſuch a perfect coincidence between them, that he thought himſelf juſtified in declaring this creature a natural idiot.

But a few philoſophers ſtill oppoſed themſelves to a deciſion ſo precipitate and ſevere. They thought it poſſible that the ſolitary and brutal life of the ſavage of Aveyron might have produced a ſort of habitual idiotiſm, the appearances of which might be ſimilar to thoſe of natural idiotiſm; and they held it extremely unjuſt to condemn the creature for ever, and extremely unwiſe to leave ſo extraordinary a phænomenon totally unexplained; at the ſame time they ſuggeſted the means that they conceived would be moſt effectual to rouſe his faculties and unfold his underſtanding, if in reality he had any. Locke and Condilhac had already given the idea of theſe means. Previous to any attempt to produce connection of ideas, the ideas themſelves ſhould be created; in order to create the ideas, the attention ſhould be fixed; and, in order to fix the attention, the wants and neceſſities of the perſon ſhould be intereſted. They did not wiſh to teach him the uſe of ſigns, before he could have acquired thoſe notions which theſe terms are intended to expreſs; they wiſhed to work on his ſenſibility, to direct it to its proper objects, and, by the formation of new habits, to counteract thoſe depraved ones by which he had already been enſlaved. They ſaw that a long time would be required to excite in him attention to a world in which he was a ſtranger, and regard for objects in which he had been, and was yet, totally unintereſted: but they reſolved to apply themſelves with induſtry, and to await the effect with patience.

The boy was committed to the care of C. Ytard, phyſician of the National Inſtitution for the Deaf and Dumb, in order that, by the combination of phyſical and modern remedies, the double incapacities under which he laboured might be more effectually removed. C. Ytard's exertions have already been crowned with a degree of ſucceſs which is almoſt prodigious: he has publiſhed the particulars, which he has dedicated to the National Inſtitute.

He proceeded nearly in the following order:—The ſenſe of feeling ſeemed to be entirely paralyſed in the child: he ſhowed no ſenſibility either to heat or cold; his ſmell and taſte were plunged in a ſimilar ſleep. A repetition of warm baths ſoon unfolded his nervous ſenſibility: in a little time after, his feeling acquired a conſiderable degree of delicacy; he became nice in the choice of his food; he made uſe of a ſelection and a cleanlineſs in it to which he had been before a ſtranger: his choice was directed by the ſmell.

The eye of this child was wild and wandering: he ſaw, without doubt, but he never dwelt on the object. The loudeſt noiſes appeared ſcarcely to ſtrike his ear; a piſtol ſhot would not make him turn his head. Superficial obſervers would have concluded that he was deaf; but C. Ytard was aware that, even when the ſenſe is perfect, no perception is produced unleſs the mind is attentive; and he was not aſtoniſhed that the violence of this ſound made no impreſſion on a being whom it could not intereſt. He found a new proof of the juſtneſs of his obſervation in the attention which his pupil beſtowed on the ſmalleſt ſound which could intereſt him, ſuch as the cracking of a nut, or the turning of a key.

In the mean time new habits were formed in the boy; a number of new neceſſities aroſe; food, dreſs, reſt, and walking out, were ſo many new means of augmenting his dependance. Finding himſelf under the neceſſity of availing himſelf of thoſe about him, he has begun to feel the force of moral affections, and has conceived a particular attachment to his governeſs. His ideas have been multiplied and connected; ſome efforts have been made to amuſe him, and it is contrived to unite inſtruction with amuſement. He has been exerciſed at compariſons; they have accuſtomed him to compare objects with their images, and in theſe compariſons he has only been conſtrained to uſe the united powers of judgment and of memory. C. Ytard thought this a favourable moment to teach him our written characters, and he made uſe of the method employed in the inſtruction of the deaf and dumb: he wrote the name of the object on the image, and then, by effacing the image, he hoped that the name would remain connected with the remembrance of the object: but this method proved unſucceſsful. Then other means were uſed, which are detailed in C. Ytard's publication, the effect of which was as happy as could be hoped. The boy now diſtinguiſhes the characters of the alphabet, and places them in their order; he pronounces the words *lait*, *ſoupé* (not ſoup), in the common tone, and then brings the proper letters, and forms theſe words. In this manner, he every day acquires a new word; he already begins to emerge from his ignorance; he has entered on the territory of reaſon; he is in poſſeſſion of ſome of our terms of ſpeech, and will ſoon be enabled to give us ſome information reſpecting his early condition; a ſubject which, of all others, muſt be moſt intereſting to curioſity.

It muſt be obſerved, that he finds great difficulty in the formation of articulate ſounds; from the effect of long diſuſe of his organs of ſpeech, there are only a few words that he can pronounce perfectly; but it is hoped that the ſame perſeverance which conquered the firſt difficulties that ſtood in his way, will alſo help him over the others.

LUNAR RAINBOW.

About ſeven o'clock on Saturday night, the 24th October, a beautiful *lunar iris* appeared at Edinburgh in the weſtern part of the horizon. As the moon was then very near the full, and her altitude above the horizon not very great, the red, yellow, and violet colours of the *iris* could be eaſily diſtinguiſhed, and the bow approached pretty near to a complete ſemicircle. This phænomenon continued half an hour; and, as the weſtern part of the horizon was during that time covered with an obſcure haze, which rendered it excellently adapted for the reception of the colours, it preſented a moſt beautiful ſpectacle to many who had never, till then, the pleaſure of obſerving it. Ariſtotle informs us, that he was the firſt who obſerved the *lunar iris*; and adds, that they are never ſeen but at the time of full moon. It has often appeared a matter of wonder to philoſophers, how this phænomenon ſo rarely occurs: but if we conſider, that it never appears unleſs the moon be near the oppoſition—that ſhe comes into this ſituation only thirteen times a year; whereas the ſun is in a ſtate fitted for their production twice every day; and that the light of the moon is incomparably more faint than that of the ſun; our ſurpriſe will quickly vaniſh, and we ſhall rather be aſtoniſhed at the frequency of their production.

Dr. Garnett, having fitted up an elegant and commodious lecture-room in Great Marlborough-ſtreet, intends, we underſtand, to begin his lectures on natural philoſophy and chemiſtry immediately. The introductory lecture will be delivered on the 2d of November, at eight o'clock in the evening. The lectures on experimental philoſophy will be delivered every Monday and Friday, at the ſame hour; and thoſe on chemiſtry every Tueſday and Thurſday, at one o'clock P. M. The medical lectures will not commence till January 1802.

XVI. *On the Equilibrium of Arches. By Mr.* JOHN SOUTHERN, *Engineer, Soho, Birmingham**.

SIR, Soho, Birmingham, Nov. 2, 1801.

AT the preſent moment, when the grand project of throwing a ſingle arch over the Thames (propoſed by Meſſrs. Telford and Douglas) is in contemplation, whatever tends to render the theory of the equilibrium of arches more eaſily to be underſtood, will, it is reaſonable to ſuppoſe, be acceptable to many of your readers; and if you think the following paper of this deſcription, you will, by inſerting it in the Philoſophical Magazine, oblige,

Sir,
Your obedient ſervant,
Mr. Tilloch. JOHN SOUTHERN.

1. LET the right lines, fig. 1. ZBCDEF be ſituate in a vertical plane, and moveable about the angles B,C,D,E as joints, and alſo about the points Z and F; let ZB be horizontal, and let ſuch weights be laid upon the joints BCDE as will keep the whole in equilibrio, the points Z and F not yielding: required what ſuch weights are, and what the forces exerted in each line.

From any point B in the line ZB draw Bp perpendicular to that line, continue the ſaid line to any diſtance, a, whence draw the line aM parallel to Bp; by the equilibrium of forces, the weight (repreſented by the line) aM laid upon the joint B will be counterbalanced by a force aB in the direction ZB, and a force MB in the direction MB, and of courſe the point B remain at reſt. But action and reaction being equal and contrary, the force aB will not only be exerted againſt B, but alſo be returned and act againſt the point Z; alſo the force MB will be returned, and act againſt the joint C. Continue BC to b, ſo that $bC = MB$, and from b draw the line bN parallel to Bp, meeting the line CD in N; then will the force bC, in the direction BC, be juſt balanced by a weight bN laid upon the joint C, and a force NC in the direction NC. In like manner continue the lines CD and DE to c and d reſpectively, making the lines $cD = NC$, and $dE = OD$; and from the points c and d drawing lines parallel to Bp meeting the line DE and EF in O

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* Communicated by the Author.

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and P; ſo will the weight cO laid upon the joint D, and the force OD, balance the force cD in the direction CD; and the weight dP laid upon the joint E, and the force PE balance the force dE acting in the direction DE. The force PE will, by its reaction, be exerted upon the point F.

Make $BA = aB$, and draw Am parallel to BC; Bm will therefore $= aM$, and $Am = BM$; and conſequently the lines AB, Bm, mA, will repreſent the forces and their directions of action which keep the joint B in equilibrium. Draw An parallel to DC, then becauſe $bC = MB = mA$, and bN is parallel to mn (included in Bp), the triangle Amn is equal and ſimilar to the triangle CbN, and conſequently the lines Am, mn, nA, repreſent the forces and their directions of action which keep the joint C in equilibrium. So, Ao and Ap, being reſpectively drawn parallel to DE and EF, the triangle $Ano = DcO$, and $Aop = EdP$; and conſequently their ſides repreſent the forces which keep the joints D and E reſpectively in equilibrium.

2. It is obvious that the weight preſſing on any joint is as the *difference of the tangents* of the angles which are formed by each of the legs of that joint with the horizontal line: for the weight on the joint D is as $cO = no$; but Bn is the tangent of the angle nAB (to the radius AB) = the angle which the leg DC of the joint forms with the horizontal line AB; and Bo is the tangent of the angle oAB (to the ſame radius) = the angle which the other leg DE of the joint forms with the horizontal line AB; conſequently, no is the difference of theſe tangents.

3. It is equally obvious that the force exerted through any leg of any of the joints, and which may be called the *compreſſing force*, is *as the ſecant* of the angle which that leg forms with the horizontal line: for the force through the leg FE is as pA, the force through ED is as oA, through DC as nA, through CB as mA, and through BA as BA; reſpectively the ſecants of the angles, with the radius AB, which each leg makes with the horizontal.

4. The horizontal force, or compreſſing force at the crown, (which is conſtant,) is AB = radius; becauſe the leg at the crown is horizontal, and therefore the angle is nothing there, the ſecant of which is equal to radius.

5. If the weights which are diſpoſed upon any *number of contiguous joints* be united and laid upon one joint, and if the legs which form this joint have the ſame declination from the horizontal line, and the ſame forces exerted through them that the *extreme* legs of the two extreme joints of the ſaid contiguous number have, the equilibrium will ſtill be exact.

Continue the line BC to G and *e*, and the line FE to G; make G*e* = MB = the force acting towards G from B; draw *eg* parallel to B*p*. By conſtruction, G*e* is parallel and equal to A*m*, and *g*G is parallel to *p*A; therefore the triangles G*eg* and A*mp* are equal and ſimilar, and repreſent the forces which keep the joint G at reſt; but the force or weight *eg* = *mp* = *mn* + *no* + *op* is equal to the ſum of the weights upon the angles CDE; and, by conſtruction, the legs FG and BG of the joint G have the ſame declination from the horizontal that the extreme legs (FE,BC) of the two extreme joints (EC) of the contiguous joints CDE have.

6. It is hence evident that the *ſum* of the weights upon *any number* of ſucceſſive joints (whether they be numerous or few), beginning at the vertex, is *as the tangent* of the angle formed by the *ultimate* leg of the laſt joint with the horizontal line.

7. Conſequently, if the legs be extremely numerous and ſhort, ſo as to form a curve, or a line differing inſenſibly from a curve, the whole weight preſſing on that curve muſt be *as the tangent of the angle* which a tangent to the curve at the loweſt point makes with the horizontal line, becauſe the ſaid tangent will coincide with the ultimate leg of the curve.

8. If the curve be the arch of a circle, the *angle*, which a tangent to the curve forms with the horizontal line, will be equal to that at the centre, contained between the vertical radius and that drawn to the point whence the tangent is drawn, and may be meaſured by the arch itſelf.

Let *adf*, fig. 2. be the arch of a circle from the centre *z*, and *za* vertical; draw *a*F horizontal, a tangent to the arch at *a*, which is conſequently at right angles to the radius *az*. From any point in the arch, as *e*, draw *em* a tangent to the curve at *e*; the angle *em*F is therefore that which the tangent to that point makes with the horizontal line; and becauſe *em*, one of its legs, is at right angles to *ez*, and F*m*, (the other leg,) by conſtruction, at right angles to *az*, the angle *eza* is equal to the angle *em*F, and may be meaſured on the arch from *a* to *e*. So the angle *dza* may be proved to be equal to that which a tangent to the arch at *d* would, if drawn, make with the horizontal. And ſo of any other point.

9. Let the arch *adf*, in the laſt figure, repreſent the intrados of a bridge; if it be required what weights ſhould be laid upon any given portions of it to effect the equilibrium, draw through the given points *bcdef* radii from the centre *z* to the horizontal line *a*F; ſo are the portions *a*B,BC,CD, DE, and EF, proportional to the weights required to be laid

on the reſpective portions ab, bc, cd, de, and ef, of the arch. For Ea is (7.) proportional to the whole weight on the arch ea, Da is proportional to the whole weight on the arch da, and therefore ED is proportional to the weight on that portion of the arch ed correſponding with it, and is as the difference of the tangents of the angles formed by tangents to the arch at the points e and d with the horizontal. (2.)

10. The horizontal force, or compreſſing force at the crown, is as radius (4.) $= za$; the compreſſing force at any point, c or d, is as zC and zD reſpectively (3.); and, of courſe, the preſſure on the abutment in direction of a tangent to the arch there, as zF.

11. It is eaſy, from what has hitherto been demonſtrated, to calculate from tables of tangents the actual weights which ſhould be laid upon given portions of arches, provided the weight upon any one portion be given. Suppoſe, for inſtance, an arch to contain from the ſummit or crown to the abutment 30°, and that on the firſt degree 10 tons be laid, (including the weight of the arch, road, &c.) then ſay, As the tangent of one degree is to 10 tons, ſo is the tangent of two degrees to the weight upon two degrees, from which ſubtract the weight upon the firſt, and there will remain the weight due to the ſecond degree; and ſo on.

Thus, as tang.	1°	co. ar.	1.75808
is to	10 tons		1.00000
ſo is tang.	2°		8.54308
Weight upon 2°	20,006 tons		1.30116
Subtract	10 the weight upon the firſt degree.		
	10.006 tons upon the ſecond degree.		

Again, as tang.	1°	co. ar.	1.75808
is to	10 tons		1.00000
ſo is tang.	15°		9.42805
Weight upon 15°	153.51 tons		2.18613

As tang.	1°	co. ar.	1.75808
is to	10 tons		1.00000
ſo is tang.	16°		9.45750
Weight upon 16°	164.28 tons		2.21558
Subtract weight upon 15°	153.51 tons		
Weight upon the 16th degree	10.77 tons		

Again,

Again, as tang.	1°	co. ar.	1.75808
is to	10 tons	-	1.00000
ſo is tang.	30°	- -	9.76144
The whole weight upon 30° = 330.77 tons		-	2.51952

Double this will be the whole weight of the bridge.

12. It was ſhown (3.) that the compreſſing force of any leg in a ſyſtem of angles was as the ſecant of the angle which that leg formed with the horizontal line. The leg at any point of a curve will coincide with the tangent at the ſame point, and therefore the compreſſing force at any point of the arch of a circle is as the ſecant of the arch between that point and the vertex (8.) Thus the compreſſing force at 30° from the crown, is as the ſecant of 30°; and this is the force exerted upon the abutment in direction of the tangent to the arch there. The compreſſing force at 2° from the crown is as the ſecant of 2°, and at the crown (10.) as the ſecant of 0 degrees.

Thus, as the tang. of	1°	- co. ar.	1.75808
is to	10 tons	-	1.00000
ſo is ſecant of	0°	- -	10.00000
The compreſſing force at the crown = the *horizontal* force, or thruſt, or puſh, againſt the abutment	572.90 tons	-	2.75808

Again, as the tang. of	1°	- co. ar.	1.75808
is to	10 tons	-	1.00000
ſo is ſecant of	30° *	-	10.06247
The compreſſing force exerted againſt the abutment in direction of the tangent to the arch at that point	661.53 tons	-	2.82055

13. This ſeems to be all that is requiſite to be known of the theory of the equilibrium of arches of a circle, ſo far as it relates to practice: the builder will naturally deſire to conſtruct his bridge with the feweſt materials conſiſtent with ſtability, and will probably firſt determine the dimenſions and weight of thoſe at the crown, which he will fix at a minimum, and from that, by the rules juſt given, the weight

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* To ſome of thoſe who may not have a logarithmic table of ſecants it may be uſeful to ſay, that the logarithmic ſecant of an angle may be had by ſubtracting the co-ſine of the angle from 20.00000.

over other parts of the arch: in moſt caſes wherein the ſpan of the arch is large, the ſpandrils will require to be hollow, and the weights, deduced as above, will ſhow him how much. To carry the theory a little further,

14. Let A*m*B, fig. 3, be any curve equilibrated by the wall ABCD built upon it, and ſuſtained by a horizontal force applied at A the crown, and a tangential one at B; let S be the centre of curvature at A and SAD vertical. It was ſhown (9. 10.) that the weight of incumbent matter on any portion of the arch of a circle, is to the horizontal force as the tangent of that portion of the arch is to radius. Let A*n* be a very ſmall portion of the curve, which may therefore be ſuppoſed not to differ ſenſibly from the arch of a circle of the radius SA, nor indeed from the tangent of that arch. Therefore the weight upon A*m* will be to the horizontal force as A*m* is to radius AS: but as the weight is proportionate to the altitude AD of the wall, ſo is the horizontal force alſo as that altitude; and as the former may be expounded by the ſmall area A*n* (*mn* being parallel to AD) = A*m* × AD, ſo may the latter be expounded by AS × AD: in other words, the horizontal force, puſh, or thruſt, is equal to a wall whoſe length is AS and altitude AD. Make BG a tangent to the curve at B, and draw BP horizontal; alſo draw *sad* parallel to SD, and make *sa* = SA, and *ad* = AD; draw *ab* and *dc* horizontal, and *sb* at right angles to BG. The angle *bsa* is therefore equal to the angle GBP, which is the angle made by the curve at B with the horizontal line; and as *ab* is the tangent of this angle with the radius *as* (= AS), the area *abcd* is equal to ABCD (7.) = *ab* × *ad*.

Draw FI parallel and very near to BC, and FH a tangent to the curve at F; alſo at right angles to FH draw *sf*, and from *f* parallel to *bc* draw *fi*. It follows, from what has been ſaid, that the ſmall trapezium BI is equal to the ſmall parallelogram *bi*.

15. Hence the *general equation* for the altitude of the wall above of all curves of equilibration, which are ſo poiſed by the matter acting upon them *vertically only*, as if ſtanding on vertical pillars cloſe together upon the arch, viz. $a\dot{\imath} = w\dot{y}$; wherein

a = height of the wall at the crown.

$\dot{\imath}$ = the fluxion of the tangent of the angle GBP, formed by the tangent to any point of the curve (as B) with the horizontal line, the radius being AS.

w = the altitude of the wall above the ſaid point (as BC), which determines the figure of the extrados.

$\dot y$ = the fluxion of the ordinate to the ſame point of the curve = the horizontal diſtance of the lines BC and FI.

16. Let the curve be the arch of a circle, and let it be required to find w. Let r = radius, and $\dot y$, $\dot z$, and $\dot t$, the cotemporary fluxions of the ſine, the arch, and the tangent, (ſee fig. 4.); alſo, let s = the ſecant, and u be drawn parallel to $\dot z$.

then, $\dot y : \dot z :: r\ (= AS) : s\ (= Sb)$

$\dot z : u :: r : s$

$u : \dot t :: r : s$

therefore, $\dot y : \dot t :: r^3 : s^3$ and $\dot t = \frac{s^3 \dot y}{r^3}$

Subſtitute this value for $\dot t$ in the general equation above, and it will be,

$\frac{as^3 \dot y}{r^3} = w\dot y$; therefore

$w = \frac{as^3}{r^3}$.

$\frac{a}{r^3}$ being a conſtant quantity, w is proportional to s^3, as found by others.

17. Let the curve be the parabola, fig. 5.

A the vertex or crown.

B the point on which w is required.

BK a tangent to the curve at B.

x = AP (and by the nature of the parabola $2x$ = PK).

y = PB; other letters as before.

$$y : 2x :: r : t$$

$$t = \frac{2xr}{y}$$

$$\dot t = \frac{2ry\dot x - 2rx\dot y}{y^2};$$

and ſubſtituting this value for $\dot t$ in the general equation $a\dot t = w\dot y$), we have

$$\frac{2ary\dot x - 2arx\dot y}{y^2} = w\dot y, \text{ hence } wy^2\dot y = 2ary\dot x - 2arx\dot y.$$

By the nature of the figure $y^2 = px$, where p the parameter = $2r$ (r being the radius of curvature at its vertex)

therefore $y^2 = 2rx$ and $x = \frac{y^2}{2r}$;

alſo $\dot x = \frac{y\dot y}{r}$.

Subſtituting theſe values in the equation juſt above,

$$wy^2\dot{y} = 2ay\dot{y} - ay^2\dot{y}$$

$w = a$; ſo that the extrados and intrados are preciſely ſimilar and equal figures, having their vertical diſtance conſtantly the ſame $= a$, the altitude of the wall at the crown.

18. Let the extrados be a horizontal line DC, fig. 6, required the intrados, a being given, and x, y, w, as before.

By the figure $w = a + x$, and the general equation will be

$$w\dot{y} = \overline{a+x}\,\dot{y} = a\dot{t};$$

$$\text{therefore } \dot{y} = \frac{a\dot{t}}{a+x}.$$

$$\text{Alſo, } \dot{y} : \dot{x} \text{ (ſee figure) } :: r : t.$$

$$\text{Therefore } \dot{y} = \frac{r\dot{x}}{t} = \frac{a\dot{t}}{a+x}$$

$$at\dot{t} = ar\dot{x} + rx\dot{x},$$

$$at^2 = 2arx + rx^2,$$

$$t^2 = \frac{r}{a} \times \overline{2ax + x^2},$$

$$t = \frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + x^2},$$

$$\dot{y} = \frac{r\dot{x}}{t} \text{ (as above) } = \frac{r\dot{x}}{\frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + xx}} =$$

$$= \sqrt{ar}\,\frac{\dot{x}}{\sqrt{2ax + xx}},$$

$$y = \sqrt{ar} \times \text{hyp. log. of } \overline{a + x + \sqrt{2ax + xx}} + \text{C}.$$

But when $x = 0$, $y = 0$, and $\text{C} = -\sqrt{ar} \times \text{hyp. log.}\, a$.

The correct fluent is therefore

$$y = \sqrt{ar} \times \text{hyp. log. } \frac{a + x + \sqrt{2ax + xx}}{a},$$

which is the ſame that Dr. Hutton makes it, page 44, new edition of his Principles of Bridges, wherein Q there $= ar$, here $=$ the horizontal force.

If a, x, and y, be given in any caſe, as, for inſtance, the altitude of the wall at the crown above the intrados and the altitude and half ſpan of the arch; then r may be found as follows, and thence the ordinates to any other point.

$$\text{Let} \quad c = \text{hyp. log.} \left(\frac{a + x + \sqrt{2ax + xx}}{a}\right).$$

$$\text{Then } y = \sqrt{ar} \times c,$$

$$y^2 = arc^2,$$

$$r = \frac{y^2}{ac^2}.$$

Example. Suppoſe the ſpan of the arch to be 100, the altitude of it 40, and the height of the wall above the crown 6 (ſame caſe as Dr. Hutton's, page 47, Principles of Bridges),

$a = 6$, $x = 40$, $y = 50$, therefore

$\frac{a + x + \sqrt{2ax + xx}}{a} = 15.268$, the hyperbolic logarithm of which is 2.7257 * $= c$, $c^2 = 7.4297$, $ac^2 = 44.578$, and $\frac{y^2}{ac^2} = \frac{2500}{44.578} = 56.081 = r =$ the radius of curvature of the arch at the crown.

19. The WEIGHT, of the incumbent matter on a curve loaded to the equilibrium, contained between the crown and any point B, fig. 3. and which may be expounded by the area AC of the wall above it, has been ſhown (14.) to be always $= ab \times ad = at$. Let A = ſaid area AC.

20. If the curve be the arch of a circle, and T = the *tabular* tangent of the number of degrees contained in the arch, then will $t = r\text{T}$, and conſequently $\text{A} = ar\text{T}$.

21. If the curve be the parabola, ſee fig. 5;

$$\left.\begin{array}{l} \text{then } y : 2x :: r : t = \dfrac{2xr}{y} \\ \text{and } y^2 = px = 2rx \quad - \quad - \end{array}\right\} \text{by the nature of the figure.}$$

$$\text{Therefore } t \left(= \frac{2rx}{y} = \frac{y^2}{y}\right) = y,$$

$$\text{and } \text{A} = ya,$$

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* The hyperbolic logarithm of a number may be found by multiplying the common or Briggs's logarithm of the ſame number by 2.302585, &c, the product is the required hyperbolic logarithm.

22. If the top or extrados be a horizontal ſtraight line, ſee fig. 6;

$$t = \frac{r^{\frac{1}{2}}}{a^{\frac{1}{2}}}\sqrt{2ax + xx},\ (18.)\ \text{therefore}$$

$$ta = \mathrm{A} = r^{\frac{1}{2}} a^{\frac{1}{2}} \sqrt{2ax + xx} = \sqrt{rax \times \overline{2a + x}}.$$

Example. Take the caſe of Dr. Hutton's laſt mentioned, wherein r was found equal to 56.081; therefore

$$rax = 13459,\ \text{and}\ 2a + x = 52.$$

$$\sqrt{13459 \times 52} = 836.60 = \mathrm{A}.$$

23. It is well known to mechanicians that if a heavy body be ſuſtained by two forces, their directions muſt meet either at the centre of gravity of that body, or in a *vertical* line which paſſes through it. Let A, fig. 7, be the body, g its centre of gravity; ea a ſtring by which it is ſuſpended, which again is ſuſtained by the ſtrings ab, ac. ac is of neceſſity vertical, and, if continued, muſt paſs through the centre of gravity g of the body. If this ſtring were either longer or ſhorter, the point a would ſtill be at reſt, and the ſtrings ab and ac under the ſame tenſion. If the body were removed vertically to the ſituation dotted, it would be ſuſtained in the ſame poſition by the ſtrings bf and cg, which would ſuffer the ſame tenſion as when they were united in the point a. If inſtead of ſtrings, props hi, kl, applied in the ſame directions on oppoſite ſides of the body, were ſubſtituted; or if, inſtead of theſe props, others parallel to them were applied, which, if continued, would meet in the vertical line, (as mn and op, which meet in g, do,) the body would ſtill be ſupported at reſt and in the ſame poſition, and the props would ſuffer the ſame force of compreſſion in theſe latter caſes, as the ſtrings on the oppoſite ſides ſuffered tenſion in the former.

24. Hence the CENTRE OF GRAVITY of the weights upon the joints E, D, C, (fig. 1.) muſt be in a vertical line paſſing through G, becauſe they are ſuſtained in equilibrium, or at reſt, by two forces in the direction BC and FE, which lines, continued, meet in G. Alſo, the centre of gravity of all the weights, ſay on B, C, D, and E, muſt be in a vertical line that paſſes through b, becauſe they are ſuſtained by two forces in the direction FE and ZB, which, continued, meet in b.

25. Hence alſo the centre of gravity of the materials upon a curve in equilibrium will be in a vertical line that paſſes through the point of interſection of the tangents to the extreme points of the curve.

Let *abc*, fig. 8. be the curve loaded to the equilibrium; let *ad* be a tangent to the curve at *a*, *cd* a tangent to the curve at *c*, and *be* a tangent to the curve at *b* (the crown). The centre of gravity of the whole materials is in a vertical line which paſſes through *d*; and the centre of gravity of the materials over the arch *a*,*b*, is in a vertical line that paſſes through the point *e*; becauſe the points *d* and *e* are reſpectively the points of interſection of the tangents, drawn from the extreme points of the portions in queſtion of the curve.

26. To find the horizontal diſtance *af* of the centre of gravity of the materials contained between the *crown* and the abutment, from the latter. Drop a vertical line *ef*, from *e*, upon the ordinate y. Let $af = d$, and $\dot{x}$ and $\dot{y}$ be parallel to, and cotemporary fluxions of, x and y reſpectively. Then it is evident that $\dot{x} : \dot{y} :: x\ (= ef) : d\ (= af)$;

therefore generally $d = \dfrac{x\dot{y}}{\dot{x}}$.

27. If the curve be the arch of a circle, *e*,*b* (ſee laſt figure) is evidently the tangent of half the arch, which ſubtracted from half the ſpan, leaves d, = ſine of the arch — tangent of half the arch.

28. If the curve be the parabola,

$$y : 2x :: \dot{y} : \dot{x} = \frac{2x\dot{y}}{y},$$ by the nature of that fig.

therefore $d = \dfrac{x\dot{y}}{\frac{2x\dot{y}}{y}} = \frac{1}{2}y$.

29. If the curve be the equilibrial curve with a horizontal extrados.

It was ſhown (18) that $\dot{y} = \sqrt{ar}\left(\dfrac{\dot{x}}{\sqrt{2ax+xx}}\right) = \sqrt{\left(\dfrac{ar}{2ax+xx}\right)}\,\dot{x}$;

therefore $d\ \left(= \dfrac{x\dot{y}}{\dot{x}}\right) = \sqrt{\dfrac{arxx}{2ax+xx}} = \sqrt{\dfrac{arx}{2a+x}}$.

Example. Take the caſe of Dr. Hutton's mentioned before, wherein r was found $= 56.081$, $rax = 13459$, and $2a + x = 52$,

$$d = \sqrt{\frac{13459}{52}} = 16.088.$$

XVII. *Obſervations and Experiments upon Dr.* JAMES'S *Powder, with a Method of preparing, in the humid Way, a ſimilar Subſtance.* *By* RICHARD CHENEVIX, *Eſq. F.R.S. M.R.I.A.*[*]

AFTER the obſervations and experiments made by Doctor Pearſon, to inveſtigate the nature of Doctor James's powder, and preſented by him to this ſociety, very little remained to be effected or deſired towards a further knowledge of the ſubject. But thoſe very experiments ſerved to ſuggeſt, that the mode of preparation was far from being the beſt that the preſent improved ſtate of chemical knowledge might afford; and that, in all probability, a leſs defective compoſition might reſult from a proceſs more conformable to ſome improvements, which of late have been advantageouſly applied to pharmaceutic chemiſtry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or ſynthetical, fire (that potent and once believed to be univerſal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and conſtant ſucceſs. And, if it is ſtill recurred to, the advantage of promptneſs, and a remnant of antient cuſtom, are the principal reaſons. But, where other methods can be deviſed to effect the ſame combinations, (and the humid way offers many,) every perſon converſant in chemical knowledge will allow the benefit of adopting them. The recent improvement in the mode of preparing calomel, is a ſtriking example of ſuch ſalutary corrections being ſucceſsfully introduced.

A few obſervations upon the formula according to which Dr. James's powder, or the *pulvis antimonialis*, is prepared, and upon ſome properties of antimony, will place this aſſertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartſhorn ſhavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phoſphate of lime, which already contains a great exceſs of lime, and add to it an oxide of antimony. In this proceſs, it has been ſuppoſed, that the phoſphoric acid of the bone or hartſhorn will ſaturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little ſulphuric acid might, during the proceſs, have been formed by the combuſtion of the ſulphur of the crude antimony, is diſſipated at a much lower temperature than that to which the powder is expoſed.

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[*] From the *Philoſophical Tranſactions for* 1801.

Every oxide of antimony with which we are acquainted is volatile at a high degree of heat: it would therefore be hazardous to aſſert, that it is poſſible to preſerve always the ſame proportion of antimony, whatever care may be employed in directing the operation; and a diſſimilarity in the chemical reſult muſt neceſſarily be attended with uncertainty in the medical application.

To this property may be added another, no leſs conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great meaſure, inſoluble in all the acids. What the effect of the gaſtric juice may be upon a ſubſtance which reſiſts the action even of nitro-muriatic acid, it is not my purpoſe to determine. It is ſufficient for me to ſay, that, as the quantity of inſoluble matter in a given quantity of Dr. James's powder, prepared at different times, may vary, the effect of any doſe alſo may differ according to the proportions of ſoluble and inſoluble matter.

I look upon it as a fortunate circumſtance, that thoſe experiments and obſervations which I mentioned in the beginning of this paper, exiſted as a ſtandard to which I might refer my own attempts, and by which I might eſtimate their validity. Dr. Pearſon has proved, (as by my own experiments I have found,) that in Dr. James's powder about 28 per cent. reſiſted the action of every acid. In examining ſome of the *pulvis antimonialis* of the London pharmacopœia, I found the average quantity of inſoluble matter to be about 44 per cent. This proportion, however, was liable to conſiderable variation [*].

The powder here treated of is denominated by Dr. Pearſon a triple ſalt, or a real ternary combination of a double baſis (lime and antimony,) with phoſphoric acid. What I have mentioned with regard to the quantity of acid contained in bone or hartſhorn, as being too ſmall to ſaturate a new portion of theſe baſes, may throw ſome doubts upon the poſſibility of any ſuch combination in the preſent caſe. But I have made ſome more direct experiments, which tend to prove that no ſuch combination does exiſt.

I took ſome white oxide of antimony, (formerly called

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[*] I find, from the information of ſeveral medical gentlemen, that the *pulvis antimonialis* is generally conſidered as ſtronger than Dr. James's powder. This ſeems rather extraordinary, when we conſider that the quantity of inſoluble matter is greater in the former than in the latter; and would almoſt lead us to ſuſpect it to be the active part of the medicine.

algaroth powder,) precipitated by water from muriate of antimony, and heated it for a long time with phoſphoric acid. I decanted the liquor, and waſhed the powder that remained. No antimony could be found in the liquor, nor could any traces of phoſphoric acid be detected in the reſiduary oxide of antimony. I then took a ſolution of muriate of antimony, and divided it into two equal parts: into one I poured diſtilled water; and into the other, a ſolution of phoſphate of ſoda. In each liquor, a copious precipitate was formed; which precipitates, after being well waſhed, were dried. The weight of both was the ſame; whereas it is evident that, had any phoſphoric acid been combined with the oxide, there would have been an augmentation of weight in that which was precipitated by the ſolution of phoſphate of ſoda. This precipitate likewiſe, upon examination, gave no traces of phoſphoric acid. From theſe experiments it appears that there exiſts no combination which can be denominated a phoſphate of antimony.

To attempt an explanation of the real nature of the powder here ſpoken of, I had recourſe to ſome experiments of M. Berthollet. By detonating ſulphuret of antimony and nitrate of potaſh in a crucible, he obtained a maſs, which he reduced to powder, and waſhed. The liquor gave, upon evaporation, a cryſtallized ſalt, which M. Berthollet terms an *antimoniate of potaſh.* I never could ſucceed in any attempt to form a ſimilar combination between the above white oxide of antimony and potaſh, owing, I believe, to the ſmall quantity of oxygen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore ſay, that the powder in queſtion is, in any degree, what M. Berthollet would call an *antimoniate of lime.*

But be the ſtate, whether of mixture or of combination, what it may, my purpoſe is to endeavour to produce a ſubſtance which, from its more certain mode of preparation, may be more equal and conſtant in its effects.

Diſſolve, together or ſeparately, in the leaſt poſſible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phoſphate of lime[*]. Pour this ſolution

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[*] In order to procure the phoſphate of lime, I diſſolved, in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its ſtate of greateſt cauſticity. By this means, the exceſs of muriatic acid, which held in ſolution the phoſphate of lime, is ſaturated, and the phoſphate is precipitated; but no muriate of lime is decompoſed, if the ammonia is quite free from carbonic acid. This is the moſt direct method of obtaining phoſphate of lime pure. This ſalt is not decompoſed, as ſome have aſſerted, by muriatic acid, but merely diſſolved by it. I have been induced

tion gradually into diſtilled water, previouſly alkalizated by a ſufficient quantity of ammonia. A white and abundant precipitate will take place, which, well waſhed and dried, is the ſubſtitute I propoſe for Dr. James's powder.

The theory of this precipitation is ſo clear and ſimple that it does not require any comment. It may be uſeful, however, to thoſe who wiſh to make this preparation, to remark, that it is abſolutely neceſſary that the ſolution of phoſphate of lime and of oxide of antimony, in muriatic acid, ſhould, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, ſhould the alkaline liquor be poured *into the acid ſolution*, the water of the former would act upon the entire maſs of oxide of antimony, while the alkali would precipitate the phoſphate of lime only as it ſaturated the acid which held that ſalt in ſolution: thus the precipitate would contain more antimony in the beginning; and, towards the end, the phoſphate of lime would be predominant. For the ſame reaſon, too, a pure alkali is preferable to its carbonate; for the carbonic acid diſengaged would retain in ſolution a portion of phoſphate of lime.

Whether this compoſition be a chemical combination or a mixture, I will not take upon me to determine; but, for the reaſons above mentioned, in ſpeaking of Dr. James's powder, I belive it to be merely a very intimate mixture. At all events, it muſt be more homogeneous than any that can be prepared in the dry way. It is entirely ſoluble in every acid that can diſſolve either phoſphate of lime or oxide of antimony ſeparately; and, to have it conſtantly and uniformly the ſame, no farther addreſs in preparing it is required, than to avoid the errors I have mentioned.

As, after ſome medical trials of the powder, it was ſuggeſted to me, that it might be advantageous to render it ſomewhat ſtronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phoſphate of lime, and precipitating as above deſcribed. The medicinal power was then conſiderably increaſed.

Dr. James's powder is a medicine which has been ſo long in uſe, and is ſo deſervedly ranked among the moſt valuable we poſſeſs, that every attempt to render the proceſs for preparing it more ſimple and more certain, muſt be allowed to be of

duced to ſtate fully theſe particulars, becauſe, from the beneficial effects of this ſalt in the treatment of rachitis, as propoſed by M. Bonhomme, (*Annales de Chimie*, vol. xviii. p. 113,) it may become of general uſe. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the ſhops.

ſome importance. But whatever reaſon there was to think, by arguing upon its chemical properties, that I had really ſucceeded in improving its medicinal virtues, it ſtill remained to be proved, by actual experiment, that the hoped-for ſucceſs was not merely conjectural. To aſcertain this, I gave ſome of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy; gentlemen whoſe extenſive practice and acknowledged ſkill ſufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. James's powder and the *pulvis antimonialis*; but that it is more mild, and conſequently may be given in larger quantities, ſeldom producing nauſea or vomiting in doſes of leſs than eight or ten grains.

XVIII. *Obſervations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid; and of this latter contained in Sulphates in general. By* RICHARD CHENEVIX, *Eſq. F.R.S. M.R.I.A.*[*]

IN a paper which I had the honour to preſent to the Royal Society of London, and the ſubject of which was the analyſis of ſome arſeniates of copper and of iron, I had occaſion, in examining many pyrites, matrices of thoſe ores, to remark the very great inequality which prevailed in the reſults of repeated experiments, made with a view to determine the proportion of ſulphur. But I ſoon perceived that the inaccuracy was cauſed by a partial combuſtion and acidification of the radical, through the means of the nitric acid employed to diſſolve the ore.

Having therefore, in the uſual manner, aſcertained what quantity of that ingredient remained untouched, I was forced to ſeek the reſt of it in the liquor which had waſhed the various precipitates. To obtain it, I poured a ſolution of nitrate of barytes into thoſe waſhings when all the other ſubſtances had been carefully ſeparated, and was thereby enabled to precipitate, in a ſtate of purity, the ſulphate of barytes, formed by that earth, and by the portion of ſulphur originally acidified in the firſt treatment of the ore by nitric acid. To come at the knowledge of the proportion of ſulphur contained in a given quantity of ſulphate of barytes, I had recourſe, in the firſt inſtance, to the quantity of ſulphur ſaid by Lavoiſier to be contained in ſulphuric acid; and, in the next, to the proportions of the latter, announced in the ſynoptic tables of Fourcroy, as entering into the compoſition of ſulphate of barytes. According to the former of thoſe chemiſts, 100 parts of ſulphuric acid contain 71 of ſulphur and 29 of oxygen; and again, in adopting the proportions of the latter, we have 33 per cent. of acid in ſulphate of barytes. But if 100 contain 71 of ſulphur, 33 muſt contain 23.43. Conſequently, for every 100 parts of ſulphate of barytes, I was to allow 23.43 of ſulphur. But, by the reſults of my analyſes, I had ſuch quantities of ſulphate of barytes as induced me to doubt the accuracy of one or other of the ſtatements by which I eſtimated the quantity of real ſulphur contained in the ore.

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[*] From the *Tranſactions of the Royal Iriſh Academy.*

No perſon is better acquainted than our celebrated preſident with the many difficulties that occur in the analyſis of ſalts in general, particularly with regard to the quantity of real acid they may contain. It has been a work of trouble to the ableſt chemiſts, and they have not always agreed in their reſults. The proportions announced by Fourcroy may therefore be doubted, in common with thoſe of the other learned operators to whom I have alluded.

The real quantity of acid produced by the combuſtion of any acidifiable baſis, can be determined by one or other of the following methods only: by direct combination in ſome ſalt, the proportions of which are already known; or by obtaining, in a ſtate perfectly free from water, the acid reſulting from ſuch combuſtion. To the former method, the general objections againſt all analyſes of ſalts muſt apply. The latter is ſtill more defective. It is by no means certain that we have ever yet obtained any acid in a ſtate of perfect ſiccity, unleſs we except the phoſphoric and the arſenic; for even the cryſtallized vegetable acids retain a portion of water in their cryſtallization. It is not that I abſolutely deny our having obtained them ſo; but I ſay merely that we have no proof. It would indeed be ſetting narrow bounds to the perfection of nature to aſſert, that no combuſtible body could, when ſaturated with oxygen, aſſume of itſelf the ſtate of liquidity; or that the oxide of the particular ſubſtance called *hydrogen* muſt be preſent to confer that property. Doubtleſs ſulphuric acid may, as well as water, contain in itſelf ſo juſt a proportion of ſpecific heat, as to remain liquid at the temperature of our globe, and under the preſſure of our atmoſphere. But both water and ſulphuric acid being eaſily volatilized, and having a powerful affinity for each other, it is not eaſy, if even poſſible, by diſtillation, to ſeparate them with ſufficient accuracy in experiments of delicate inquiry.

A ſecond ſource of error, therefore, remained open in this branch of the calculation, which gave the proportion of 23.43 of ſulphur, as that contained in 100 parts of ſulphate of barytes.

However, before I would allow myſelf to call in queſtion ſuch authorities as thoſe I have quoted, I inſtituted the following experiments:—In a tubulated glaſs retort I put 100 parts of purified ſulphur, and poured upon them ſtrong nitric acid. A quilled receiver, plunging into a Woulfe's apparatus, was adapted to the retort; and all being well luted, I proceeded to diſtil. The liquor which came over was poured back ſeveral times upon the ſulphur, until the whole was diſſolved. The water which had come over, and that through which the nitrous gas, produced during the operation, had paſſed, were eſſayed for ſulphureous acid, and no traces of it could be found. No ſulphur had been volatilized, therefore no ſuſpicion could remain that all was not converted into ſulphuric acid. The liquors which were in the various parts of the apparatus were united, and to them was added a ſufficient quantity of nitrate of barytes. The whole was evaporated gently; becauſe, though I am well acquainted with the very little ſolubility of ſulphate of barytes, I well know that nitric acid will retain a ſmall portion of it, particularly when formed in a liquor where that acid abounds. In a firſt experiment I obtained 694 from 100 of ſulphur; in a ſecond, 348 from 50; and, in a third, 347 from the ſame quantity. But the ſimple rule of three reduced theſe quantities to 14.6 or 14.4 per cent. of ſulphur contained in ſulphate of barytes; a difference wholly to be neglected. If, therefore, we take 14.5 as the average for the quantity of ſulphur contained in 100 parts of ſulphate of barytes, we ſhall not be far from the truth. From the accordance of theſe experiments, repeated and varied, I had now no doubt but concerning the ſource where I was to ſeek the error, which gave 23.43 as the juſt proportion.

To aſcertain this point, I operated in the following manner:—I prepared ſome lime as pure, I believe, as chemical means can procure it. I digeſted white marble in muriatic acid; and, by leaving an exceſs of the earth, was certain that, by the ſuperior affinity of lime for that acid, nothing elſe had been taken up. Upon trying the ſolution with ammonia, no precipitate took place. By means of carbonate of potaſh, I ſeparated the lime in the ſtate of carbonate; and, after well waſhing the precipitate, expoſed it in a platina crucible to a violent heat till the weight no longer diminiſhed. I am acquainted with no more efficacious method to prepare lime fit for the delicate purpoſes of ſcientific chemiſtry.

One hundred parts of this lime were diſſolved in dilute muriatic acid, in the ſame platina crucible, previouſly weighed; and then ſulphuric acid was added in ſufficient quantity. Sulphate of lime was precipitated; and the veſſel was expoſed to a heat, at firſt gentle, to evaporate the liquor; and then, by degrees, raiſed to a temperature which could expel every thing but the combined ſulphuric acid, and leave the ſulphate of lime completely calcined. The crucible with the ſalt was then weighed, and the augmentation was 76. It appears to me, that, if we admit (and I ſee no reaſon that we ſhould not admit it) that calcined lime and calcined ſulphate of lime are wholly exempt from water, it muſt be clear that the 76 additional weight were ſulphuric acid; and that the ſulphuric acid muſt in this ſtate, more than in any other, approach nearer to what may be termed abſolutely real acid. One hundred parts of calcined ſulphate of lime contain, therefore,

Lime - -	57
Sulphuric acid -	43
	100

By the former experiments (thoſe made upon ſulphur converted into acid, and then united to barytes,) we had the quantity of ſulphur contained in ſulphate of barytes. By the latter (thoſe made by directly combining lime with ſulphuric acid) we had the proportion of real acid contained in calcined ſulphate of lime. Conſequently, by knowing the ratio that ſulphate of barytes bears to ſulphate of lime, with regard to the acid in each, we ſhall arrive at the knowledge of the quantity of ſulphur contained in real ſulphuric acid. For this purpoſe, I attempted to diſſolve, in water, 100 parts of ſulphate of lime. But finding in this method of proceeding a conſiderable inconvenience ariſing from the great quantity of liquor neceſſary to effect the ſolution of that ſalt, I had recourſe to the following expedient:—Upon 100 grains of calcined ſulphate of lime, I poured ſome oxalic acid, which attracts the baſis with an affinity ſuperior to that exerciſed by ſulphuric acid. Oxalate of lime was here formed; but oxalate of lime is ſoluble in a very ſmall exceſs of any acid. A little muriatic acid operated a complete ſolution; and thus a great quantity of ſulphate of lime required but little water to diſſolve it. Into this liquor, muriate of barytes was poured, and ſuffered to remain ſome time, gently heated. By theſe means any oxalate of barytes that might have been formed, was retained in ſolution by the original exceſs of acid; and the entire quantity of ſulphate of barytes was depoſited. Of the exactneſs of all theſe methods, which I uſed as the inſtruments by which I aſcertained theſe reſults, I convinced myſelf by various preliminary experiments. After the uſual filtration, waſhing and drying at the gentle heat of a ſand-bath, I obtained in one experiment 185, in another 183, and, laſtly, in another 180. This difference does not exceed the limits of what all perſons converſant in analytic chemiſtry will allow to experiments of this nature. We may therefore take 183 as the mean proportion; conſequently we ſhall ſay, that 183 of ſulphate of barytes contain the ſame quantity of ſulphuric acid as 100 of ſulphate of lime; and 183 : 43 : : 100 : 23.5. Therefore 23.5 is the proportion of acid in 100 of ſulphate of barytes. But we have before ſeen that 14.5 of ſulphur, acidified by nitric acid, form that portion of ſulphuric acid contained in 100 of ſulphate of barytes, viz. 23.5. We muſt now ſay, that 23.5 : 14.5 :: 100 : 61.5, and the fourth term will be the proportion of ſulphur = 61.5, which combined with 38.5 of oxygen will form 100 of real ſulphuric acid.

In neither of the proportions, whether it be of the acid contained in the ſalt, or of the combuſtible baſis contained in the acid, do I agree with the two chemiſts whom I have quoted. This juſtly excited ſome doubts in my mind, and led me to repeat my experiments. Nor ſhould I yet be thoroughly ſatisfied, if I could not, upon other grounds than by ſuppoſing inaccuracy in them, account for the apparent differences. We muſt ever expect to ſee the errors of our predeceſſors corrected by men much inferior in abilities, but who, by poſſeſſing more certain means, ſupply the want of genius and invention. At the time in which the experiments were made that determined the proportion of 33 per cent. of ſulphuric acid in ſulphate of barytes, it was not known that we had never obtained any barytes pure; and that a conſiderable portion of carbonic acid reſiſted the action of every degree of heat that had been applied to carbonate of barytes. The fact was, I believe, firſt obſerved by Pelletier, but the method of avoiding the inconvenience was pointed out by Vauquelin. He decompoſes nitrate of barytes by fire, and a moderate degree of heat is ſufficient to expel all the acid and the water. The chemiſts I have mentioned performed ſynthetic experiments by combining, directly or indirectly, ſulphuric acid, and ſuch barytes as they imagined to be pure. The conſtant ſimilarity of their reſults is ſufficient to prove the accuracy of their operations; but, working upon an impure ſubſtance, they muſt have been contented with a ſimilarity of error.

Three cauſes may exiſt which are capable of accounting for any variation, whether in *plus* or in *minus*, that might have appeared in the experiments, by which Lavoiſier determined the quantity of ſulphuric acid obtained by the combuſtion of ſulphur in oxygen gas.

1ſt, A part of the ſulphur may be volatilized during combuſtion.

2d, All the ſulphur may not be converted into ſulphuric acid, but part may remain in the ſtate of ſulphureous acid.

3d, In rectifying, ſome acid may come over along with the water; or, *vice verſâ*, ſome water remain with the acid. Theſe conſiderations will excuſe me for having propoſed a doubt, where the authority of ſo great a man exiſts againſt the experiments which I have related.

The method which I had uſed to aſcertain the quantity of ſulphur in an ore had been practiſed by Taſſaert (*Annales de Chimie*, No. 82: Analyſis of Cobalt from Tunaberg); but he calculated according to the proportions of Lavoiſier and of Fourcroy. In another paper by Thenard, (*Annales de Chimie*, No. 96,) he ſtates the proportions of ſulphuric acid, obtained by treating ſulphur with nitric acid, to be

Sulphur - -	55.56
Oxygen - -	44.44
	100.00

but in the extract given by Guyton in that number of the *Annales de Chimie*, the mode of operation is not deſcribed. Calcined ſulphate of barytes is eſtimated in the ſame paper to contain

Barytes - -	74.82
Sulphuric acid -	25.18
	100.00

which proportions are as near to what I had found as can be expected; for ſulphate of barytes does not contain more than 3 per cent. of water of cryſtallization, and they muſt be deducted from the quintal.

Having determined with accuracy the proportion of acid in any inſoluble ſulphate, it is eaſy to proceed to the determination of that contained in any other ſulphate. The docimaſtic art, or analytic chemiſtry, in general, cannot however expect to derive ſuch advantages from the knowledge of ſoluble ſalts, as of thoſe which, from their inſolubility, may be uſed with accuracy, in delicate experiments, to determine the proportions of the conſtituent parts of bodies.

But if with this mode of operating we compare the quantities of real acid, ſaid (in thoſe excellent tables with which Mr. Kirwan has enriched the ſcience) to be contained in ſulphuric acid of different ſpecific gravities, each will ſerve as a proof of the validity of the other; and perhaps demonſtrate, that ſulphuric acid, without the intervention of water, may enjoy liquidity at the temperature and preſſure which act upon our globe.

XIX. *Experiments on Platina.* By L. Proust.

[Continued from p. 55.]

XIII. *Of the Cryſtallization of Solutions of Platina.*

THE ſmall yellow, red, and ſand-like grains which are depoſited at the bottom of the united ſolutions, and which ſometimes aſſume the octaedral form, are generally taken for muriate of platina. The quantity of theſe cryſtals, which is always very ſmall, does not increaſe by concentration; which, however, ought to be the caſe, if theſe cryſtals were really muriate of platina. No more of them appear in ſolutions from which they have been once ſeparated; and their ſeparation is even complete a long time before the moment when the real muriate of platina begins to cryſtallize.

Theſe cryſtals are a double ſalt, compoſed of muriate of platina and potaſh, as will be ſeen hereafter: they are formed from the ſmall quantity of potaſh which, by the inadvertency of the workmen, remains in the neck of the retorts, when preparing nitric acid, and from that alſo which is carried over in the diſtillation itſelf; for nitre is always found, after the rectification of aquafortis, at the bottom of the retorts. The greater part of thoſe who have operated on platina having in general employed the acids ſold in the ſhops, we need not be ſurpriſed that they ſhould have found cryſtalline depoſits of the kind here alluded to, and that none of them are ever ſeen in ſolutions made on purpoſe with acids well purified.

If ſome of theſe cryſtals be heated in a glaſs tube cloſed at one end, their nature may be ſoon diſtinguiſhed: becauſe the muriatic acid, charged with the oxygen of the metal, is volatilized in gas; while the platina, reduced to the metallic ſtate, and mixed with the muriate of potaſh, remains at the bottom of the tube.

Lewis, in my opinion, is the only perſon who has accurately obſerved the cryſtallization of platina. The ſolution was accidentally at that degree of ſaturation which could conduct to this diſcovery; and this circumſtance, as will be ſeen, is the firſt of thoſe neceſſary for the ſucceſs of the cryſtallization in queſtion.

The ſecond condition conſiſts in not leaving an exceſs of acid; and this Lewis accompliſhed; for he ſays (p. 148) that the ſavour of theſe cryſtals was harſh and diſagreeable, but much leſs corroſive than he expected, on account of the great quantity of acid which he had employed. Indeed, if this ſolution be prepared in the ſame manner as prepared by Lewis, it will be found to have the ſavour of ſour vegetables; and whatever diſagreeable taſte it has, ariſes from the iron and copper.

The muriate of platina, when pure, eaſily cryſtallizes; but it is difficult to make it cryſtallize when it is united with foreign muriates not ſuſceptible of cryſtallization, and which oppoſe this tendency. In this there is nothing which diſtinguiſhes it from the moſt cryſtallizable ſalts, and therefore we muſt endeavour to diſcover means for diſengaging it from theſe obſtacles. We ſhall now examine thoſe which may remove the exceſs of acid from a ſolution of platina.

Every ſolution which has been ſubjected to ebullition and concentration is not charged with muriatic acid, and the cauſe may be eaſily perceived: but the caſe is not the ſame with the nitric acid, which ſuffers itſelf to be concentrated; and though it is not attracted by the oxides, which the muriatic acid ſeizes upon excluſively, it adheres to them with ſufficient force; but if it be difficult to expel it, it may be eaſily decompoſed.

To accompliſh this, a little muriatic acid and crude platina muſt be added to the ſolution before it be diſtilled. This acid, which cannot act upon the platina until it has been oxidated, at the expenſe of the nitric acid, determines the decompoſition of the latter, and facilitates the ſeparation in the form of nitrous gas. This operation may be repeated, and when no more gas appears you may be aſſured that you have a ſolution diſengaged from every exceſs of acid. When the ſolution is brought to this term, nothing but muriates different from that of platina can retard the cryſtallization: theſe muriates are thoſe of copper and iron oxydated to a *maximum.*

To ſeparate them, it is indiſpenſably neceſſary to concentrate the ſolution in ſuch a manner, that it may form in cooling a maſs, which moulds itſelf in the retort, but of ſuch a conſiſtence, that, when placed on its ſide, it ſhall ſuffer a portion of the liquor to eſcape. If the concentration has not come to that point, and if the reſiduum preſents only a thick ſubſtance like honey, it muſt be again placed over the fire. But if it has been carried too far; that is to ſay, if the ſeparation of the liquid part cannot be effected, you muſt then add to the maſs, which we here ſuppoſe to be four or five pounds, ſome ſpoonfuls of water, and rediſſolve it by the help of heat and ebullition. This method, which is very eaſy for thoſe acquainted with the practice of the laboratory, is the ſame as that employed for refining ſugar, and ſeparating from it the molaſſes.

After having ſurmounted this obſtacle, the reſt becomes eaſy. Nothing then is neceſſary but to add a quantity of water nearly equal to the volume of the muriates that have thus been ſeparated; to expoſe the whole to heat; to ſuffer the retort to cool in the furnace; and to proceed to two or three ſucceſſive diſtillations, to ſeparate the cryſtals of the muriate of platina.

This method has a double object—the cryſtallization of the muriate, and its purification. It is evident, and cannot be doubted, that, by repeating this proceſs a ſufficient number of times, platina may be as completely purified as by any of the other means hitherto known. But while we admit all the utility of this method, we muſt allow that it is tedious and troubleſome, both in regard to the cryſtallization of the muriate, and its purification. The others therefore, in our opinion, ought to be preferred; for, to obtain the cryſtals with the greateſt eaſe, it will be ſufficient to concentrate a ſolution of platina purified by a proceſs which we ſhall ſpeak of hereafter. If any one, however, wiſhes to carry this method of purification to the higheſt degree, he may be certain of having attained to it by making the following experiment:

Diſſolve, in a certain quantity of water, a little of this muriate, and try it with ammonia. If the common yellow precipitate of the red oxide of iron be obtained, there ſtill remains ſome muriate of the latter combined with the platina; but the muriate of platina is pure if no precipitate manifeſts itſelf.

It is of importance that the ſolution ſhould be well diluted, otherwiſe the ammonia would form with the platina that kind of ſaline precipitate which is produced by the muriate of ammonia.

At the moment of their concentration theſe ſolutions of platina experience a bubbling, which would endanger the retort if the perſon who conducts the operation perſiſted in continuing the fire. Theſe movements ariſe from the union of a yellow powder, which as yet is only the potaſhed muriate of platina of which we have juſt ſpoken, and which it is indiſpenſably neceſſary to ſeparate. In regard to the moſt expeditious means of heating a retort which contains 12 or 15 pounds of matter, and of maintaining the heat, a ſupport of wire ought to be preferred to ſand-baths, which I have long baniſhed from my laboratory, even in the preparation of æther, the rectification of acids, &c.

But when it is neceſſary to operate on a great ſcale with a certain number of veſſels, it is evident that ſand-baths placed in a row in a gallery will in this caſe be attended with more certainty.

XIV. *Of the Compoſition of the Nitro-muriatic Acid for the Solution of crude Platina.*

No author has ſaid any thing poſitive in regard to the proportions which will yield the ſtrongeſt nitro-muriatic acid for this kind of labour. This uncertainty, which ſtopped me during my firſt reſearches, induced me to endeavour to diſcover theſe proportions by direct experiments.

As the ſtrength of acids varies in the different manufactories, it appeared to me proper to take them at that degree of concentration at which they are commonly found in the ſhops. I confined myſelf therefore to aquafortis of 35 degrees by Baumé's areometer, and muriatic acid of 15 degrees, without giving myſelf any trouble about the nitric acid which might be preſent in the muriatic acid, or the iron and the ſulphuric and ſulphurous acids with which common muriatic acid is often contaminated: for, if I had employed in my reſearches rectified acids, it is evident that my reſults could not be applied but with reſtriction to operations on platina on a large ſcale, in which caſe it will always be neceſſary to employ the acids of commerce.

Exper. I. One pound of nitro-muriatic acid, compoſed of
8 ounces of nitric acid and
8 ounces of muriatic acid,
diſſolved 13 drams of platina out of 24 which I employed.

Exper. II. Nitro-muriatic acid, compoſed of
8 ounces of nitric acid and
10 ounces of muriatic acid,
diſſolved 15 drams of platina.

Exper. III. An acid, compoſed of
4 ounces of nitric acid and
10 ounces of muriatic acid,
diſſolved 17 drams and 54 grains of platina.

Exper. IV. An acid, compoſed of
2 ounces of nitric acid and
14 ounces of muriatic acid,
diſſolved 13 drams and 38 grains of platina.

Theſe four experiments were made at the ſame degree of heat; that is to ſay, in four retorts, diſpoſed according to my mode on wire grates, and placed on a furnace. That the reſults may be more eaſily compared, I ſhall here give them in the following table:

Exper.	Black crude platina.	Nitric acid of 35°	Muriatic acid of 15°	Platina diſſolved.
I.	6 ounces	8 ounces	8 ounces	13 drams
II.	6 ounces	6 ounces	10 ounces	15 drams 24 grains
III.	6 ounces	4 ounces	12 ounces	17 drams 54 grains
IV.	6 ounces	2 ounces	14 ounces	13 drams 38 grains

Let us now ſee the conſequences which may be deduced from this table.

1ſt, In the three firſt experiments it is obſerved that the quantity of platina diſſolved increaſes in proportion to that of the muriatic acid, from which it reſults that this acid is the real ſolvent of platina.

2d, If we ſuppoſe that the twelve ounces of muriatic acid, found in the four ounces of nitric acid the quantity of oxygen neceſſary for diſſolving the platina, it is ſeen that a conſiderable exceſs of the latter was uſeleſsly expended in the two preceding experiments; that is to ſay, 4 ounces too much in the firſt, and 2 in the ſecond; and, by the ſame reaſon, the ſolutions of the firſt and ſecond experiments were more acid than thoſe of the third and fourth.

3d, Acids, the ſtrength of which is in a ſimilar proportion, or nearly three parts of muriatic acid for one of nitric acid, muſt form the propereſt proportion for diſſolving crude platina, and conſequently purified platina.

4th, The fourth experiment proves to us that the nitric acid, which was in the proportion of only 1-7th of the muriatic acid, was not able to furniſh to the latter the quantity of oxygen neceſſary to make it charge itſelf with all the platina which it ought to diſſolve; hence it reſults, that in this laſt proportion there is a leſs ſolution of platina, and a loſs of muriatic acid.

5th, As the muriatic acid conſtitutes the ſtrongeſt part of the ſolvent of crude platina, in the commencement of the labour care muſt be taken that the heat does not attain to the point of ebullition; becauſe, 1ſt, At that temperature the muriatic acid eaſily acquires aëriform expanſion; 2d, The tendency which it has to ſeparate itſelf from the mixture is ſtill ſtronger when it begins to be oxygenated. From what I have obſerved, the ſolution ought never to begin at a heat ſuperior to 60 or 70 degrees of Reaumur (135° to 157° Fahr.), and it is then effected, without loſs of acid, with all the rapidity poſſible. In a word, the liquor ought not to be boiled till the effervеſcence decreaſes.

The five conſequences which we have here deduced, and which are nothing but the reſult of the ſame experiments, give us the true principles of the art of diſſolving platina.

If we now compare the proportion of platina with the nitro-muriatic acid, we ſhall find that the quantity diſſolved riſes ſomewhat above the eighth part of the weight of the ſolvent. This proportion, however, is ſubject to ſome variation; for the ſame acid employed in ſimilar circumſtances diſſolves ſometimes a leſs quantity, though in general it deviates very little from this proportion.

XV. *On the Utility which may be derived from the Products of this Diſtillation.*

In operations on a large ſcale, œconomy being one of the moſt important objects, it may be readily conceived that the acid products in queſtion ought to be carefully collected, and that the concentration ſhould be carried as far as poſſible to increaſe theſe products; for the expenſe of theſe ſolutions will be ſo much more moderate, if the ſame quantity of acids can be employed for a greater quantity of platina. That I might be better enabled to aſcertain the advantages in this reſpect, I made the following experiment:

I diſtilled, to the conſiſtence of concrete honey, a ſolution ſimilar to that of the third experiment, the product of which I carefully collected; and by theſe means I was enabled to obſerve in this operation three different periods.

In the *firſt* the diſtillation, which began at a heat of nearly 70 degrees (or about 150° Fahr.), was accompanied with effervеſcence and a diſengagement of nitrous gas, which, as Lavoiſier obſerves, deſerves to be examined, and eſpecially the moment when the oxide of azote or azotic oxide manifeſts itſelf.

In the *ſecond period*, the effervеſcence was followed by ebullition; and here no nitrous gas was diſtinguiſhed; a weak colourleſs acid only is ſeen to riſe, while the ſolution makes no other progreſs than that of approaching towards concentration.

When the heat had made the greater part of the ſuperfluous liquids paſs over into the receiver, the nitrous gas again ſhowed itſelf; and it appeared to me at this *third period,* that the portion of the free acids, which had attained to a certain degree of concentration, exerciſed a reaction on the crude platina. But I ſoon perceived that I could not depend on this advantage; for, after having diluted in water the ſaline maſs, to ſeparate the exceſs of the crude platina, I found that the pound of nitro-muriatic acid had diſſolved only 16 drams and 12 grains of platina; a quantity which differs by more than a dram from that of the third experiment: but notwithſtanding this variation, it is not, as I have ſaid, an eighth above the other.

The nitrous gas of this third period gives us reaſon to believe that it does not differ in its origin from that obſerved in the concentration of metallic nitrates, when the latter approach condenſation. The augmentation of heat, therefore, only decompoſes a remainder of the nitric acid, from which the red vapours in queſtion reſult.

Let us now return to the diſtillation. The liquor found in the receiver weighed 10 ounces and 1 dram: it had no colour, and by the areometer indicated only 9 degrees; while the nitro-muriatic acid, which had produced it, indicated 22 before it was employed. The liquor is ſometimes ſeen ſlightly coloured, though the operation may have been conducted with the uſual precautions; becauſe the movement of the ebullition is no doubt ſufficient to throw a few drops of the ſolution into the neck of the retort. This, on the whole, is only a little platina, as is indicated at the moment by the cinnamon-coloured precipitate, occaſioned by the hydro-ſulphurated water. Theſe acid products have no ſenſible action on the platina; but, to know the advantage which ought to be taken of them, I divided them into two parts, of five ounces and half a dram each.

I added to one of theſe parts 2 ounces of muriatic acid, to the other an equal quantity of nitric acid; and theſe two new mixtures were treated, as uſual, with three ounces of the ſame crude platina.

The firſt diſſolved 4 drams and 16 grains of platina, which would have approached 8½ drams if I had employed the 10 ounces and 1 dram of the product with the addition of 4 ounces of muriatic acid.

The ſecond could diſſolve only 48 grains, that is to ſay, 1 dram and 24 grains, had I employed the ſame product with the addition of 4 ounces of nitric acid. Let us now examine the conſequences which may be deduced from theſe reſults.

If we ſuppoſe that the firſt mixture could diſſolve 8 drams and a half of platina, the muriatic acid added muſt have found a ſufficient quantity of nitric acid to oxygenate it; but 4 ounces of my muriatic acid not being able to diſſolve ſo ſtrong a doſe of platina, it thence follows that, in the total product of the diſtillation, there muſt have been preſent two ounces, at leaſt, of the ſame acid. Since 12 ounces of muriatic acid were indeed employed in the third experiment to diſſolve 17 drams of platina, it is evident that to diſſolve the half of this quantity, or 8½ drams, 6 ounces of the ſame acid would have been required; and ſince we had in the receiver the quantity of 2 ounces of muriatic acid, there is no doubt that there remained in the retort about 10: in a word, that theſe 10 ounces of acid at 15 degrees, ſtrengthened by the oxygen of the nitric acid, can only diſſolve 16 or 17 drams of black crude platina.

Let us now proceed to a valuation of the nitric acid which might be contained in the product of our diſtillation.

It is ſeen by the third experiment that 12 ounces of muriatic acid require not leſs than 4 ounces of nitric acid to enable it to diſſolve 17 drams of platina. There were therefore 2 ounces at leaſt in the product of our diſtillation, ſince, with the aſſiſtance of 4 ounces of muriatic acid added, it could have diſſolved 8½ drams of platina, or the half of 17.

It thence follows, that there paſs into the receiver, during the whole courſe of the diſtillation made with the proportions of the third experiment, nearly 2 ounces of nitric acid at 35°, and an equal quantity of muriatic at 15°, diluted in 6 ounces 1 dram of water; which quantities, united, form the product, being 10 ounces 1 dram. It thence reſults alſo, that the 17 drams were really diſſolved by 10 ounces of muriatic acid, oxygenated by the decompoſition of only 2 out of the 4 ounces of nitric acid employed. Should it however be aſked, on ſeeing this eſtimate, why 16 or 17 drams of platina could not be diſſolved in a mixture of 10 ounces of muriatic acid and 2 ounces of nitric acid? we reply, that the heat neceſſary to aſſiſt the action of this ſolvent may have ſoon changed the proportions of the compoſition of the acid, as in the experiment we have analyſed.

In regard to the firſt acid, formed by the product of the diſtillation, it is certain that it contains a ſufficient quantity of nitric acid to oxygenate the 2 ounces of muriatic acid which we found in it: but the great quantity of water in which they are diluted deſtroys all their action; and if the addition of 4 ounces of nitric acid reſtores to them ſome ſtrength, it is only, in all probability, becauſe that quantity of acid augments, to a certain degree, the concentration of the muriatic acid.

From theſe data let us try to determine what is the largeſt quantity of platina that can be diſſolved in nitro-muriatic acid prepared according to the proportions above indicated.

To diſſolve firſt the 17 drams and 54 grains, and then the 8 drams and 32 grains, making together 26 drams and 14 grains, we employed 12 ounces of muriatic acid and 4 of nitric acid, and afterwards 4 ounces of the former, that is to ſay, 20 ounces of mixture in the whole. Calculation ſhows, neglecting the 14 grains of platina, more or leſs, that only 5 pounds 11 ounces[*] of nitro-muriatic acid are requiſite to diſſolve a pound of black crude platina, which is that I employed in theſe experiments. If we conſult the chemiſts, we ſhall find that the greater part of them have fixed at 16 parts the quantity of nitro-muriatic acid which muſt be employed to diſſolve one of platina. If I am not miſtaken, Lewis and count Sickingen were the only perſons able to diſſolve it with 8 parts of ſolvent. But they were obliged to perform two operations to diſſolve that quantity. White crude platina is the kind moſt ſoluble: for I find, in a note, that a pound of nitro-muriatic acid, compoſed according to the proportions indicated, diſſolved 18 drams and 24 grains.

XVI. *Solution of Platina by the Means of a Nitro-muriatic Acid compoſed of Nitric Acid*[†] *at* 31° *and Marine Salt.*

Bergman found that an acid compoſed in this manner diſſolved platina exceedingly well. It will be ſeen by the following experiments that, in an operation on a great ſcale, this ſolvent may be preferred to thoſe compoſed with pure acids. To be able to diſcover the moſt advantageous proportion of acid and ſalt, I made a ſeries of experiments, but I ſhall give an account of thoſe only which can throw light on ſolutions of this nature. As the method of employing this kind of nitro-muriatic acid is not different from that followed by others, I ſhall immediately proceed to a comparative table of the reſults:

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[*] We are here, no doubt, to underſtand the Spaniſh pound, which is nearly a ſeventeenth leſs than the French pound; but as it is divided into 16 ounces, or 128 drams of 72 grains, the proportions remain the ſame.

[†] The degrees here mentioned are thoſe of Baumé's areometer; 31° indicate a ſpecific gravity of 1·279.

Exper.	Black crude platina.	Nitric acid at 35°	Marine ſalt.	Platina diſſolved.
I.	6 ounces	6 ounces	6 ounces	20 drams 48 grains
II.	6 ounces	16 ounces	7 ounces	24 drams 12 grains
III.	6 ounces	16 ounces	8 ounces	21 drams 52 grains
IV.	6 ounces	16 ounces	9 ounces	21 drams 36 grains

As the reſult of No. 2. appeared to me the moſt important, I thought it my duty to repeat it, and I obtained 24 drams 16 grains of diſſolved platina.

In No. 4. there was an exceſs of ſea ſalt, which had attached itſelf to what remained of the crude platina.

I made my experiments with purified marine ſalt; but as in operations on a large ſcale the common kind is employed, I repeated them with common ſalt, and found the ſame reſults in equal quantities of the ſolvent; with the ſame variation, however, in the quantity of the platina diſſolved, which might have been remarked in the operation performed with the acids. I repeated alſo the ſame trials, making ſome changes in the two laſt columns:

Exper.	Black crude platina.	Acid at N° 35.	Common ſalt.	Platina diſſolved.
I.	6 ounces	16 ounces	6 ounces	20 drams
II.	6 ounces	16 ounces	8 ounces	22 drams
III.	6 ounces	16 ounces	10 ounces	20 drams
IV.	6 ounces	16 ounces	12 ounces	19 drams

An exceſs of marine ſalt was found here alſo in Nos. 3 and 4.

It is ſeen by theſe two tables, that the quantity of platina diſſolved experiences a diminution whether the proportion of 7 ounces of ſalt per pound of acid be increaſed or diminiſhed. It however appears, that in the experiments where more than 7 ounces were employed, the quantity of platina diſſolved ought to have been, if not greater, at leaſt equal; ſince the nitric acid ought to have diſengaged and oxygenated the ſame quantity of muriatic acid. I confeſs that I cannot conceive how an exceſs of marine ſalt or nitrate of ſoda, found in the mixture, could prevent the ſolution of a greater quantity of crude platina, unleſs the explanation of this phænomenon be found in an obſervation I ſhall here make, or my occupations allow me to undertake new reſearches on this ſubject. The obſervation I allude to is as follows: The reſiduum of all the ſolutions, that is to ſay, the remaining crude platina, exhibits an aſpect very different from that of the reſiduums left by nitro-muriatic acid compoſed of free acids. [The author promiſes a continuation of theſe experiments.]

XX. *On the Analyſis of Wine.* *By* C. Chaptal[*].

In all wines we diſtinguiſh an acid, alcohol, tartar, extractive matter, aroma, and a colouring principle; the whole diluted or diſſolved in a portion of water more or leſs abundant.

1ſt, *Acid.*—An acid exiſts in all wines; I never found any which did not preſent ſome traces of it. The ſweeteſt and moſt luſcious wines redden teſt paper, if ſuffered to remain in them for ſome time; but they are not all acid in the ſame degree. There are ſome the principal character of which is a natural acidity. Thoſe made from grapes not perfectly ripe, or produced in cold climates, are of this ſort; while thoſe produced by the fermentation of very ripe and ſaccharine grapes exhibit very little acid. The acid, then, appears to be in the inverſe ratio of the alcohol, which is the reſult of the decompoſition of the ſugar.

This acid exiſts in great abundance in verjuice; and is found in muſt, though in a ſmaller quantity. All fermented liquors, ſuch as cyder, perry, and beer, as well as fermented farinaceous ſubſtances, alſo contain this acid: and I have found it even in molaſſes. Indeed it is to ſaturate it completely, that lime, aſhes, or other earthy or alkaline baſes, are obliged to be employed in the purification of ſugar. Without this precaution, the exiſtence of the acid would oppoſe the cryſtallization of the ſugar.

If wine be concentrated by diſtillation, the extract which reſults from it in general has a ſour and pungent taſte. To diſſolve and ſeparate the acid, nothing is neceſſary but to pour water or alcohol over the extract. This acid has a pungent taſte, an odour ſlightly empyreumatic, leaves a ſenſation of harſhneſs on the tongue, &c. When well filtered and left in a jar, it ſuffers to be precipitated a conſiderable quantity of extractive matter. It then becomes covered with mouldineſs, and in that ſtate ſeems to approach the acetous acid. It may be purified by diſtillation, which ſeparates from it a large quantity of extractive matter; and it is then leſs liable to be decompoſed by putrefaction.

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[*] From the *Annales de Chimie*, No. 109.

This acid precipitates the carbonic acid from its combinations: it diſſolves with facility the greater part of the metallic oxides: it forms inſoluble ſalts with lead, ſilver, and mercury; and ſeparates the metals from all their ſolutions in acids.

This acid forms alſo an inſoluble ſalt with lime. If abundance of lime water be mixed with wine, it precipitates the acid, which carries with it all the colouring matter. This acid, then, is of the nature of the malic acid. It is always mixed with a little of the citric acid; for, when digeſted on oxide of lead, beſides the inſoluble precipitate which is formed, there is produced a citrate, which may be ſhown in it by the common means.

This malic acid diſappears by the acetification of the wine: in well made vinegar, nothing exiſts but acetous acid. This transformation of the malic acid into acetous acid naturally explains why wine which has only begun to turn ſour cannot be employed in manufacturing acetite of lead: in this caſe there is formed an inſoluble precipitate, with the production of which I was greatly embarraſſed till I learned the reaſon of it. For a long time, C. Berard, my aſſociate in my manufactory of chemical productions, added nitric acid to ſour wine to give it the property of forming a ſoluble ſalt with lead. I was then of opinion that the acid of wine was by theſe means oxygenated, while the effect really produced was to haſten the decompoſition and converſion of the malic acid into vinegar.

The exiſtence of the malic acid in wine in different proportions, ſerves to throw ſome light on a phænomenon of the higheſt importance in regard to the diſtillation of wines, and the nature of the ſpirit produced from them. Every body knows, not only that all wines do not give the ſame quantity of ſpirit, but that the ſpirits produced from them are far from being of the ſame quality. No one is ignorant that beer, cyder, perry, and fermented farinaceous ſubſtances, give little ſpirit, and always of a bad quality. Careful and repeated diſtillation may, indeed, correct theſe faults in a certain degree, but never deſtroy them completely. Theſe conſtant reſults, eſtabliſhed by long experience, have been referred to the greater quantity of extractive matter contained in theſe weak ſpiritous liquors: the combuſtion of a part of this principle by diſtillation, ought, it would appear, to be an immediate effect of it, and the acrid and empyreumatic taſte, a very natural conſequence. But, when I examined this phænomenon more cloſely, I found that, beſides the cauſes depending on the abundance of this extractive principle, it was neceſſary to admit another, viz. the preſence of the malic acid in almoſt all theſe caſes. Having diſtilled indeed, with great care, theſe different ſpiritous liquors, I always obtained acidulous ſpirits, the taſte of which was altered by that eſſentially belonging to the malic acid: it is only by confining one's ſelf to extracting the moſt volatile liquor, that it is poſſible to ſeparate a little alcohol free from all alteration; and ſtill it retains a diſagreeable odour, which does not belong to pure ſpirit.

Wines which contain the greateſt quantity of malic acid give ſpirits of the worſt qualities. It even appears that the quantity of alcohol is leſs, as the quantity of the acid is more conſiderable. If this acid be laid hold of by means of lime water, chalk, or fixed alkali, very little alcohol can be obtained by diſtillation; and in all theſe caſes the ſpirit aſſumes a diſagreeable taſte of the fire, which does not contribute to improve the quality.

The difference of the ſpirits ariſing from the diſtillation of different kinds of wine depends then, chiefly, on the different proportions in which the malic acid is contained in theſe wines; and no certain means have yet been obtained of deſtroying the bad effect which this acid produces by its mixture with the ſpirit.

This acid, which we have found in grapes at every period of their increaſe, and which does not diſappear from wine till the moment when it degenerates completely into vinegar, would deſerve to be diſtinguiſhed by the name of the *vinous acid*; but, to avoid innovation, we ſhall retain that of the *malic acid*.

2d, *Alcohol.*—Alcohol forms the real character of wine. It is the product of the decompoſition of the ſugar, and the quantity of it is always in proportion to that of the ſugar which has been decompoſed [*].

Alcohol,

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[*] I ſhall not enter into the queſtion, whether alcohol is completely formed in wine, or whether it is a product of diſtillation; or, in other words, the reſult of fermentation or of diſtillation. Fabroni has adopted the latter ſentiment, becauſe, having mixed a hundredth part of alcohol with

new

Alcohol, then, is more or leſs abundant in all wines. Thoſe of warm climates furniſh a great deal; thoſe of cold climates give ſcarcely any at all. Ripe and ſaccharine grapes produce it in abundance; while the wines ariſing from green aqueous grapes, which contain little of the ſaccharine principle, afford very little.

There are ſome wines in the ſouth which furniſh a third of ſpirit: there are ſeveral in the north which contain only a fifteenth.

It is the proportion of alcohol that renders wines more or

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new wine, he was only able to ſeparate, by means of potaſh, the ſame quantity of alcohol. But this experiment appears to me to prove at moſt that the foreign alcohol added to the wine does not enter into ſo exact a combination as that which exiſts naturally there: it remains in the ſtate of ſimple mixture. We obſerve a ſimilar phænomenon when highly concentrated alcohol is diluted by the addition of a greater or leſs quantity of water; for it is known in commerce that this weakened alcohol has not the ſame taſte, which marks, however, the ſame degree of ſpiritoſity. I conſider alcohol, then, in wine not as exiſting there in an inſulated ſtate, and diſengaged from all combination, but as combined with the colouring principle, carbon, alkali, extractive matter, and all the other conſtituent principles of wine: ſo that wine is a ſuper-compounded whole, all the elements of which may be extracted by chemical means; and when, by the application of heat, theſe principles are ſeparated, the moſt volatile riſe firſt, and what firſt paſſes is a very light compound, forming alcohol, then water, &c.

Diſtillation, by ſucceſſively extracting all the principles of wine, according to the invariable laws of their gravity and of their affinities, breaks and deſtroys the primitive combination which conſtitutes wine, and exhibits products which, when united again, cannot reproduce the primitive body, becauſe the heat has entirely diſunited and ſeparated the compound into principles which may exiſt in an inſulated ſtate, and which have ſcarcely any affinity.

In a word, it is of little importance to art whether the alcohol exiſts or does not exiſt in wine; the diſtiller ſtill has invariable principles both in regard to the quality and to the quantity of alcohol which each kind of wine can furniſh. Whether the heat, therefore, combines the principles of the alcohol, or merely extracts them from a maſs where they are combined, the manner of operating and the reſults of the operation can receive from them no modification. We ſee in the diſtillation of all vegetable matters, and their products, a repetition of the phænomena exhibited by the diſtillation of wine.

Diſtillation by heat is not the only means of extracting the alcohol from wine. 1ſt, The carbonic acid gas which is diſengaged by fermentation carries with it, and in a ſtate of ſolution, a very conſiderable quantity of alcohol, as I have already ſhown. 2d, The gas which eſcapes from champagne carries off almoſt all the alcohol contained in that wine. 3d, Very ſpiritous wines, when ſhaken in the bottle, ſuffer to eſcape a very ſenſible flavour of alcohol. 4th, The wines which furniſh moſt ſpirit are thought to be moſt ſpiritous to the taſte. All theſe facts cannot be reconciled with the hypotheſis of the formation of alcohol by diſtillation, and ſeem to prove that it exiſts quite formed in the wine.

The reader on this important ſubject may recur to the opinion of Fourcroy, publiſhed in *Annales de Chimie.*

leſs generous; it is it which diſpoſes them to, or preſerves them from, acid degeneration. Wine turns ſour with more facility the leſs alcohol it contains, the proportion of extractive matter being ſuppoſed to be the ſame in both.

The richer wine is in ſpirit, the leſs it contains of malic acid; and this is the reaſon why the beſt wines furniſh, in general, the beſt ſpirit, becauſe they are then freed from the preſence of that acid which gives them their diſagreeable taſte.

It is by diſtilling wines that all the alcohol they contain is extracted.

The diſtillation of wine has been known for ſeveral centuries; but this operation has been ſucceſſively improved, and at preſent has been brought to ſuch a ſtate of perfection, that the ſpirit trade muſt derive great benefit from it, and that the proceſs may be applied with advantage to every other kind of diſtillation. The alembics long uſed for diſtilling were boilers with a long narrow cylindric neck, having at the ſummit a hollow hemiſphere, from which proceeded a tube, of no great ſize, deſtined to convey the liquor into the worm. Arnold de Villanova ſeems to have been the firſt who gave us any accurate notions in regard to the diſtillation of wines; and to him we are indebted for the firſt deſcription of this long-necked alembic, models of which are ſtill found among our perfumers.

The idea entertained, that the product of the diſtillation was the more delicate, ſubtile, and pure, as it was raiſed higher, by making it paſs through very ſmall tubes, directed the conſtruction of theſe ſtills. But diſtillers were ſoon convinced that it was not ſo much the obſtacles oppoſed to the aſcent of the vapours as the art of graduating the heat in a proper manner, which rendered the product of the diſtillation more or leſs pure. It has been found that, in the firſt caſe, the force of the fire changes the nature of the ſpiritous principles by communicating to them an empyreumatic taſte; while, in the ſecond, pure ſpirit ariſes, and paſſes into the worm without alteration. On the other hand, œconomy, an important conſideration in the arts, has cauſed all the changes made in the old proceſſes to be adopted.

Thus the column perpendicular to the boiler has been ſucceſſively lowered, the capital enlarged, the boiler made wider, and the following general forms have been gradually adopted.

The ſtills, at preſent, are a kind of flat-bottomed boilers, the ſides of which riſe perpendicularly to the height of about twenty-two inches. At this height a contraction takes place, which reduces the aperture to eleven or twelve inches. This aperture is terminated by a neck ſome inches in length, in which is adapted a ſmall cover called the *head*, which increaſes in diameter towards the upper part ſo as to have the form of a truncated cone inverted. From the angle of the baſe of this capital proceeds a ſmall tube deſtined to receive the vapour of the ſpirit, and to tranſmit it to the worm with which it is connected. The worm conſiſts of ſix or ſeven circumvolutions, and is placed in a caſk kept full of water to facilitate the condenſation of the vapours: theſe vapours, when condenſed, run down into a tub deſtined to receive them.

The boilers, in general, are fixed in maſon-work up to the part where the contraction takes place: the bottom alone is expoſed to the immediate action of the heat. The chimney is placed oppoſite to the fire-hole; and the aſh-hole, which is not very large, is ſeparated from the fire-place by an iron grate.

The boiler is charged with five or ſix quintals of wine. The diſtillation is completed in eight or nine hours, and during each operation about ſixty pounds of coals are conſumed.

Such is the proceſs which has been long uſed in Languedoc; but though old, and generally adopted, it preſents imperfections, which muſt ſtrike every man acquainted with the principles of diſtillation.

1ſt, The form of the boiler produces a column of liquor exceedingly high and not broad, which being expoſed to the action of the heat only by its baſe, is burnt in that part before the upper part becomes warm; bubbles then riſe from the bottom, which, being obliged to traverſe a colder maſs of liquid, are condenſed, and again diſſolve in the liquor. It is only when the whole maſs has been heated gradually that the diſtillation is eſtabliſhed.

2d, The contraction at the upper part of the boiler and the internal bending which it preſents in that place, ſtill hurt the diſtillation: for this part, not being covered by maſon-work, is continually expoſed to the air, which maintains there a colder temperature than in other parts, ſo that the vapours which riſe are condenſed partly againſt the interior ſurface, and fall down in drops, or run back in ſtriæ into the body of the ſtill. In this caſe, the ſame thing happens which we daily ſee take place in diſtilling with a ſand bath: the vapours which riſe, ſtriking againſt the uncovered ſurface of the retort, which is always the coldeſt, are condenſed, and fall down in ſtriæ into the bottom; ſo that the ſame portion of matter riſes, falls down, and is diſtilled ſeveral times; which occaſions loſs of time, expenſe in fuel, and hurts the quality of the product, which in ſome caſes is altered and decompoſed. Theſe phænomena may be rendered very ſenſible by cooling the upper part of the retort of the ſand-bath at the moment when the diſtillation is moſt active: the vapours ſucceſſively become viſible in the inſide, and drops are condenſed on the ſides, which ſoon run down and join the liquor contained in the retort.

Beſides, the contraction at the upper part of the boiler forms a kind of eolipile, where the vapours cannot paſs without difficulty, which neceſſarily requires a more conſiderable force of aſcenſion. This fact has been properly explained by Baumé.

3d, The capital is not conſtructed in the moſt advantageous manner: the head acquires almoſt the temperature of the vapours, which, being ſtrongly dilated, preſs on the liquid and confine its aſcent.

4th, The method of managing the fire is no leſs improper than the form of the apparatus: the aſh-hole is every where too ſmall, the fire-place too large, the door badly ſhut, &c. ſo that a current of air is eſtabliſhed at the door, and is precipitated into the chimney, paſſing over the coals. Conſequently, a violent fire is required to heat the boiler moderately. The grate is choked up with a thick ſtratum of combuſtibles, ſo that they become nearly uſeleſs for want of a free circulation of the air.

[To be continued.]

XXI. *Account of a Deſcent into the Crater of Mount Veſuvius by eight Frenchmen on the Night between the* 18*th and* 19*th of July* 1801.

TO aſcend to the ſummit of mount Veſuvius, which is elevated 3600 feet above the level of the ſea, is an enterpriſe of great difficulty, as it is neceſſary for nearly half the height to climb an exceedingly ſteep declivity up to the knees in aſhes. Some philoſophical men of eminence, however, as Spallanzani, Dolomieu, Dr. Moore, &c. have overcome all theſe difficulties. Sir William Hamilton, who cauſed a great many views of Veſuvius to be deſigned during his long reſidence at Naples, aſcended to the ſummit of it ſixty-two times; but no one, at leaſt ſince the eruption in 1779, ever ventured to deſcend into the crater of this volcano, not even Sir William Hamilton, who conſidered it under ſo many points of view, and who viſited it ſo many times. It was reſerved for eight Frenchmen to hazard this dangerous enterpriſe, and to ſucceed in it completely, notwithſtanding the timidity of their guides, the impoſſibility which the Neapolitans attached to it, and the inſtances they mentioned of raſh travellers who had loſt their lives in the attempt, and been ſwallowed up by the volcano.

To be able to appreciate the danger of this enterpriſe, it will be neceſſary to have a correct idea of the form and poſition of Veſuvius, and of the matters which it throws up. This volcano has the form of a truncated cone, and a part of its baſe, which is altogether three leagues in circumference, is waſhed by the Mediterranean; its mouth, or upper baſe, which is a little inclined to the axis, is 5722 feet in circumference. The earth from the baſe to half the height conſiſts of vegetable mould mixed with lava and ſtones which have not been attacked by the fire, tufas, pumice, and calcareous ſtones, different in their nature and colour according to the different degrees of impreſſion which have been made on them by the fire.

The half of the height next the ſummit is compoſed chiefly of pure aſhes, but coarſer than our common aſhes. Till the preſent time, there have been twenty-four eruptions recorded in hiſtory. The firſt took place in the year 79 after the Chriſtian æra: by theſe eruptions volcanic matters have been ſucceſſively accumulated, but by that of 1779 the ſituation of the crater and of the aperture was entirely changed. The focus or crater is now ſunk 200 feet below the upper edges of the mouth of the volcano.

To arrive at the crater, and to obſerve the numerous ſpiracles, long crevices, and fires which iſſue from them in ſeveral places, and alſo the variegated and ſtill ſmoking matters of which the crater is compoſed, it was neceſſary to paſs over this ſpace of 200 feet.

The inner ſides of the volcano are nearly perpendicular, or exceedingly ſteep, and compoſed of aſhes, lava, and large calcareous ſtones; but theſe lava and ſtones, as they form no connection with the aſhes, cannot ſerve as any point of ſupport; and when any one is ſo imprudent as to adhere to this kind of rock, the leaſt motion, the leaſt diſplacement of any part, makes the whole crumble to pieces. Beſides, from the ſummit of Veſuvius to the crater, the declivity, being exceedingly rapid, cannot be traverſed but on all fours, and ſuffering yourſelf to glide down amidſt a torrent of aſhes and lava. But the moſt dangerous obſtacles are thoſe awful excavations, which cannot be paſſed over without great trouble and difficulty.

Diſregarding the terror with which the Neapolitans endeavoured to inſpire us, after having received their adieus, as if our ſeparation had been likely to be eternal, we ſet out in a carriage, at half after eleven at night, on the 18th of July, from the hotel of the French ambaſſador, fourteen in number, furniſhed with ropes and other articles which we ſuppoſed might be neceſſary, and all in a ſtate of the higheſt ſpirits, which never forſook us, even at times of the moſt imminent danger. We arrived about midnight at the foot of Veſuvius; and, having quitted our carriage, mounted well experienced mules, and proceeding one after the other, with adjutant Dampierre at our head, amidſt the thick darkneſs of night, reached half way to the ſteep ſummit of the mountain. We had a numerous body of guides, and their lighted torches gave to our expedition a myſterious and ſolemn air, which formed a ſtriking contraſt with the mirth and gaiety of the company.

When we had aſcended about half way, we were obliged to alight, and to clamber up the ſteepeſt and moſt difficult part of Veſuvius, wading through the aſhes up to the knees, till, exhauſted with fatigue, and covered with ſweat, we reached the ſummit at half paſt two in the morning.

The firſt thing that ſtruck us, as ſoon as the morning began to dawn, was a moſt magnificent ſpectacle—a ſuperb view of the city and port of Naples, the beautiful hills which ſurround them, and the vaſt extent of the ſea by which they are waſhed. After walking round part of the aperture of the volcano, that we might chooſe the moſt commodious place for deſcending, adjutant Dampierre and C. Wickar firſt deſcended, without any accident, at the determined point. When they had got about a third of the way they were ſuddenly ſtopped by an excavation of fifty feet, which it was neceſſary to paſs. As they found that it was impoſſible to obtain any fixed point of ſupport on aſhes ſo moveable; and being convinced that the friction of ropes would have ſoon deſtroyed both the point of ſupport and the neighbouring maſſes to a great diſtance, they reſolved to return. Beſides, while deliberating on the means of deſcending, ſome ſtones rolling down from the ſummit occaſioned a general agitation wherever they paſſed: adjutant Dampierre found the ground on which he ſtood ſhake beneath his feet; and he had ſcarcely quitted it, calling out to C. Wickar to follow him, when it diſappeared. Soon after, indeed, the whole place where they had ſtood, and all the neighbouring ſmall eminences, crumbled down ſucceſſively in the courſe of half an hour, and were precipitated to the bottom of the crater with an awful noiſe.

Before we renounced our enterpriſe to return to Naples, dejected on account of not having ſucceeded, we once more walked round the mouth of the crater, and at laſt diſcovered a long declivity, pretty ſmooth though very ſteep, which conducted to the focus. Without examining the precipices which it might be neceſſary to paſs before it could be reached, C. Debeer, the ambaſſador's ſecretary, accompanied by a lazzaroni, ſet out firſt to attempt the paſſage. When they had got half way, amidſt a torrent of aſhes, which the impreſſion of their feet made to roll down along with them, they found means to fix themſelves on the edge of a precipice twelve feet in height, which it was neceſſary to paſs before they could reach the lower declivity. The lazzaroni, frightened, refuſed to proceed; but, being promiſed a double ducat, avarice got the better of his timidity; he ſpeedily made the ſign of the croſs over his whole body, and, having invoked the Madonna, and St. Anthony of Padua, threw himſelf along with C. Debeer to the bottom of the firſt precipice: ſoon after they arrived at another, but being of leſs height it was paſſed with more eaſe. At length, amidſt a continual torrent of falling lava, aſhes, and ſtones, they arrived at the bottom of the crater, and ſtretched out their arms to us, ſending forth ſhouts of joy, which we returned with the utmoſt ſatisfaction and enthuſiaſm.

C. Houdouart, engineer, immediately followed C. Debeer; and after encountering the ſame difficulties, and paſſing dangerous precipices, joined him at the bottom of the crater. Being there both convinced of the almoſt inſurmountable difficulty of aſcending, they threw themſelves into each other's arms, like two friends reduced to the neceſſity of terminating their lives together in a deſert iſland without any hopes of eſcaping from it.

They then began, but with cautious ſteps, to walk round this immenſe furnace, which ſtill ſmokes in ſeveral places. The intrepid Wickar, who was very deſirous to participate in their fate, called out to them to ſend ſome one to aſſiſt him in paſſing the two cliffs; but ſeeing no one coming, and growing impatient, he ruſhed forward, and rolled down towards them amidſt a torrent of ſtones, aſhes, and volcanic matters. Adjutant Dampierre, C. Bagneris phyſician to the army, Freſſinet and Andras French travellers, and Moulin inſpector of poſts, ſoon followed, and arrived at the crater after having incurred the ſame dangers.

Wickar immediately ſat down on a heap of ſcoriæ, and, with that ſuperiority of talents for which he is diſtinguiſhed, ſketched out in profile, with a perfect reſemblance, the portraits

traits of the eight Frenchmen who had deſcended. Each then formed a ſmall collection of the different volcanic matters which appeared to be new or curious, and endeavoured to make a few obſervations.

Had we been allowed to depend on ſucceſs, had we not been retarded in our preparations by our timid guides, and if ſome of us, having only juſt arrived at Naples, had not been ſtraitened in point of time, our deſcent would certainly have been much more uſeful, and the reſults more ſatisfactory. However, though ill furniſhed with means, the following are the obſervations we were enabled to make:

Reaumur's thermometer, the only inſtrument we poſſeſſed, ſtood at 12 degrees, on the ſummit of Veſuvius: the air was cold, and ſomewhat moiſt: in the crater the quickſilver roſe to 16 degrees, and we experienced the mildeſt temperature.

The ſurface of this place, which, when ſeen by the naked eye, looking down from above, appeared entirely ſmooth, exhibited, when we were at the bottom, nothing but a vaſt extent of aſperities. We were conſtantly obliged to paſs over lava exceedingly porous, in general pretty hard, but which in ſome places, and particularly thoſe where we entered, was ſtill ſoft, and yielded under our feet. The ſpectacle which ſtruck us moſt was the numerous ſpiracles, which either at the bottom of the crater or the interior ſides of the mountain ſuffer the vapours to eſcape. When we arrived at the crater, we were deſirous to aſcertain whether theſe vapours were of a noxious quality: we walked through them, and inſpired them ſeveral times, but felt no inconvenience from them. The thermometer placed in one of theſe ſpiracles indicated 54 degrees, in another it roſe only to 22. In all theſe experiments our inſtrument was covered with a humid matter, which was ſoon diſſipated in the open air without leaving any traces.

In traverſing the ſurface of the crater, we perceived a focus half covered by a large maſs of pumice ſtone, and which, from its whole circumference, emitted a ſtrong heat. The thermometer placed at firſt at the entrance of it, and then immerſed to as great a depth as the nature of the ground and the heat would admit, never roſe higher than 22 degrees. This ſingularity ſurpriſed us, but we were not able to explain it.

The volcanic productions which we obſerved in the whole crater were lava, exceedingly porous, and which the fire in certain places had reduced to ſcoriæ. It was of a dark brown colour, and ſometimes reddiſh, but it is rare to find any white. The ſubſtances neareſt the ſpiracles are all covered or impregnated with ſulphur. This mineral is found very often in a ſtate of oxygenation. It is ſometimes white, and ſometimes of a yellowiſh colour, and the ſharp and pungent impreſſion it leaves on the tongue ſufficiently indicates the ſtate in which it is. The burning focus, of which we have ſpoken, produces the ſame reſults. Some baſaltic lava is alſo found, but in ſmall quantity; one ſpecimen only, of a conſiderable weight and beautiful poliſh, attracted our attention.

On the north ſide of the crater there are two large fiſſures, one of which is 20 feet in depth, and the other about 15. They are ſhaped like an inverted cone. The matter with which they are covered is entirely ſimilar to that on the reſt of the ſurface. They emit neither ſmoke nor heat; yet ſome ſulphurous productions plainly ſhow that the fire in theſe places has not long been extinct.

When we had finiſhed theſe few obſervations, it was neceſſary that we ſhould think of returning. The deſcent is far leſs laborious than the aſcent; for it is difficult to climb eminences where the points of ſupport are ſo moveable. Beſides, people cannot aſcend but one at a time in ſucceſſion, after long intervals, for fear of burying under a torrent of volcanic matters thoſe who follow, as the foot, when moved, diſplaces the aſhes, &c. to the diſtance of thirty feet round.

When we arrived at the two precipices, we were obliged to aſcend by mounting on the ſhoulders of a man placed at the bottom, and laying hold of a ſtick held by another at the top, and to reſt our feet no where but in a very gentle manner. At length, by prudence and caution, we reached the ſummit of Veſuvius without any accident, but exhauſted with fatigue, and ſo covered with aſhes and ſmoke, as to be ſcarcely diſtinguiſhable. Our ſix companions, who had not deſcended into the crater, were overjoyed when they ſaw us again, and ſupplied us with ſome refreſhments, of which we had great need.

When one grand difficulty is ſurmounted, inferior ones are overlooked, as of little importance. In leſs than twenty-five minutes we again deſcended, having confirmed, after examining various ſtones, this obſervation, that Veſuvius is the only known volcano which throws up from its bowels primordial ſubſtances, without being altered by the fire, and ſuch as are found at preſent in banks and veins.

At half after eight in the morning we arrived at Portici, the inhabitants of which were much ſurpriſed to ſee us return all ſafe. Their delicious fruits, and their excellent wine called *lacryma Chriſti*, ſoon made us forget our fatigue, and we then proceeded to Naples, which we reached in ſafety.

The reſult of this excurſion, which was only an experiment, can be of no further uſe than to ſhow the poſſibility of reaching the crater, and to open the way to it to philoſophers, naturaliſts, and chemiſts, who, by exploring this immenſe furnace of nature at their leiſure, will ſind a variety of matters which will afford an ample ſield for the application of their chemical knowledge, and may enable them to make diſcoveries intereſting to the arts and the ſciences.

The names of the eight Frenchmen, in the order in which they deſcended, are as follow: Debeer, ſecretary to the ambaſſador Alquier; Houdouart, chief engineer of bridges and cauſeways attached to the army of Italy; Wickar, painter; Dampierre, adjutant-commandant; Bagneris, phyſician to the army of obſervation; Freſſinet and Andras, French travellers; and Moulin, inſpector of poſts.

XXII. *Obſervations on a deſtructive Kind of Inſects found in ſome Parts of the Ruſſian Empire, and in the Bannat of Temeſwar.* By *Profeſſor* PALLAS[*].

WE are indebted to my friend Brunnich for the firſt account of this deſtructive inſect, hitherto obſerved only in the Bannat of Temeſwar in Europe. Linnæus, in the ſhort deſcription which he gave of it [†] from an account tranſmitted to him, compares it to that ſmall gnat (*culex equinus*) which infeſts horſes. Its principal colour is blackiſh; its breaſt projects in a round form, and the hinder part of the body is oval; the legs are pretty long; the thighs towards the body are white, but black underneath; the covering of the thighs moſtly white, at the extremity blackiſh, the joints of the legs black, and the wings tranſparent.

According to this deſcription, I found the greateſt ſimilarity between the gnat of the Bannat and the Ruſſian *moſchkara*, or ſtinging gnat [‡], which, after an attentive compariſon, I find to agree perfectly with the creeping gnat (*culex reptans*) of Linnæus; and I ſuſpected, from what I knew of the abundance and deſtructive nature of the Siberian inſects, that the gnat of the Bannat might belong to the larger and more pernicious kinds in the warm mountainous diſtricts of that country. I was, however, deceived in my conjecture, by ſeveral circumſtances mentioned in Griſelini's Natural Hiſtory of the Bannat*, and his deſcription of this gnat, which is totally unlike that of Brunnich. According to Griſelini, it is larger than the common gnat, and perfectly like it in the conformation of its body; its head is alſo armed with feelers, and it has a ſmall proboſcis; the breaſt is blackiſh at the top, ſomewhat hairy, and on the lower part white; the fore legs are ſhort, and the hind largeſt; the extremity of the body conſiſts of lead-coloured rings, each of which is ſurrounded by a black line Griſelini ſays nothing of white rings on the feet, and at the end names the inſect, contrary to the expreſſion of his own deſcription, a gad-fly (*œſtrus*).

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[*] From *Neue Nordiſche Beyträge*, vol. ii.

[†] *Culex Lanio* Linn. *Mantiſſ. Plantar.* ii. p. 541.

[‡] *Bibio ſanguinarius*, Pallas *Reiſe*, part i. p. 193; Append. p. 475; by the Ruſſians called *moſchka*; in Siberia improperly *mokriza*; on the Volga *moſchkara*.

In this uncertainty, the laſt circumſtance led me to conjecture that Griſelini, perhaps, might not be a very experienced entomologiſt; and as he deſcribed his inſect in the time of harveſt, and as the real deſtructive gnats appear on the other hand in the ſpring, I ſuſpected that Profeſſor Brunnich might have deſcribed the proper inſect, and that Griſelini had only ſeen ſome of the other kinds, which appear in large ſwarms. My doubts on this ſubject induced me to apply to Baron von Born, to whoſe information I had often been before indebted; and by his means I obtained a ſmall collection of real Columbach gnats, among which I diſtinguiſhed, on the firſt view, the Ruſſian *moſchkara* of a conſiderable ſize, and even an inſect which I had ſo often wiſhed to procure when collecting plants and inſects in the Altai mountains, and on the Volga.

They are ſmall flies, as thick but much ſhorter than the common gnats, and which, according to their characteriſtic marks and form, have a greater reſemblance to the *bibiones* of Geoffroy than to gnats. Their breaſt, which is ſtrong, projects in a round form; the head is flat, and at the top ends in a ſharp oblique edge, has at the ſides oblong eyes, and below tapers to a proboſcis, which ends in two ſharp points, not unlike the ſting of the gnat; the feelers are of a browniſh yellow colour, thick and ſharp pointed, placed near each other on the forehead; the legs are much ſhorter than thoſe of the gnat; the thighs and part next the joints of the legs are all white, and only blackiſh towards the ends; on ſome the thighs towards the body were whitiſh; the extremity of the body, which appears very dry and contracted, is ſurrounded with rings of brown, but the breaſt appears black, ſhaded with grey; the wings, which are pretty large and broad, appear whitiſh, with their veins, and lie over each other.

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* *Griſelini Verſuch einer politiſchen und naturlichen Geſchichte des Temeſwarer Bannats.* Vienna, 1780; part ii. p. 125.

Such I found the Columbach gnat, and I have ſeen the Siberian *moſchara*, or that common in the neighbourhood of the Volga, perfectly ſimilar, only that it was ſomewhat ſmaller, and had not ſo broad white rings on the legs. I entertain no doubt, therefore, that they are both of the ſame ſpecies. I am alſo convinced that the *culex reptans* of Linnæus is nothing elſe than the ſame inſect which is found in the northern countries, ſomewhat ſmaller and not in ſuch abundance.

Linnæus found great multitudes of the latter in the mountainous diſtricts of Lapland, whereas in Sweden they are more uncommon. According to his obſervations, they attack people principally towards ſun-ſet, crawl over their whole bodies, even into the mouth, noſe, and eyes, and cannot be driven away like other gnats, by ſhaking or blowing. If his deſcription in the *Fauna Suecica* be compared with ours, it will be found very little different.

Theſe troubleſome flies are found here and there in the northern foreſts of Ruſſia, but ſingly, and for the moſt part they run about among the hair of the cattle. But on the Volga, below Kaſan, where that river begins to flow between woody mountains, and approaches a warmer diſtrict, particularly from the neighbourhood of Simbirſk to Saratof and Kamyſchenka, they are met with from the end of May to the beginning of June in ſuch aſtoniſhing multitudes in low buſhy places, and the woody hills ſheltered from the wind, that they ſeem to fill the atmoſphere, like hail, falling blindly and with violence againſt the face, which is affected as if ſand were thrown againſt it; they fly into the eyes, noſe, and mouth, adhere pertinaciouſly to the ſkin, and with their blunt trunks pierce it often in ſuch a manner as to occaſion pain, ſo that a bloody puncture remains, though without itching. Fiſhermen, hunters, and all thoſe who, by their employments, are expoſed to the open air, or who travel, furniſh themſelves about the above period with a large net-cap ſoaked in birch oil, becauſe it has been obſerved that the *moſchka*, however blindly they ruſh againſt every thing, never venture to fly againſt thoſe open nets which have been ſoaked in the above ſtrong-ſmelling oil. Without theſe means it would often be impoſſible to open the eyes. When the inſect has an opportunity of faſtening itſelf on the ſkin unperceived, and of ſucking the blood, it fills its belly till it appears like a blown-up bladder, and cannot be removed but by killing it. As people cannot open the mouth without ſeveral of them getting into it at once, it often happens that they bite or ſqueeze them between the teeth in ſpitting them out, and one then feels, without wiſhing it, that their entrails have a ſweet melleous taſte. At the end of June they almoſt all diſappear, and are not again ſeen till Auguſt; but at that period they are far leſs abundant.

Theſe flies are equally numerous in the Ural mountains, but they are ſtill more ſo in the neighbourhood of the woody mountains of the ſouthern part of Siberia beyond the lake Baikal. In the month of June, people are tormented with theſe vermin, even on the mountains, till they reach the cold ſummits, where there are no woods. In the latter end of ſummer, however, they are not to be ſeen in thoſe diſtricts. Travelling from Jakuſk to Ochozk, they are to be again found in immenſe multitudes as ſoon as one has paſſed the Aldaan, and perhaps they are to be found alſo in North America.

It is commonly reported, in the Siberian and Ural mountains, that theſe ſmall inſects, with the aſſiſtance of the *gad flies*, which abound about the ſame period, torment horſes, and other cattle, in ſuch a manner as to occaſion their death when they run about in the foreſts, and have no opportunity of eſcaping to the open country, or to a fire, the ſmoke of which would drive them away. Inſtances of this, however, are not ſo frequent in theſe countries as, according to every account, they muſt be in the Bannat of Temeſwar. The cauſe of this, perhaps, is to be aſcribed to the great ſize of theſe gnats in the latter country, and perhaps alſo to ſome poiſonous quality connected with their ſting; for I have obſerved, even in Siberia, that their ſting ſometimes occaſioned on the human body large tumours, which ſcarcely diſappeared at the end of forty-eight hours.

I ſhall not here repeat what Griſelini, whether true or falſe, has related of theſe inſects. I ſhould have conſidered many of the particulars which he ſtates, as containing too much of the marvellous, had not Baron Born, in his Letters, confirmed the accounts given of the deſtructive nature of theſe vermin in that country. "They come to the Bannat of Temeſwar," ſays he, "by millions, as ſoon as the bloom appears on the trees in the ſpring; fall upon the cattle, creep through the rectum, the noſtrils, and the ears, into the moſt interior parts of their bodies, and kill them in four or five hours. When the animal is opened they are found adhering in ſwarms to the lungs and to the bowels, which are completely inflamed. They continue three or four weeks, and are followed by immenſe clouds of dragon flies, *libellula* *grandis* and *ærea*, which devour every thing, and are in their turn devoured by the ſwallows, which find them in ſwarms. About the end of July or the beginning of Auguſt the ſecond brood appear, and accompanied with the like circumſtances."

XXIII. *Memorandums, Hints, Precepts, and Recipes, for the Uſe of Artiſts, Manufacturers, and others; including various ſhort Proceſſes either new or little known.*

[To be occaſionally continued.]

On ſome Kinds of Gilding.

THE value which has always been ſet on articles made of gold, has induced artiſts to deviſe means for giving to works of other metals, by covering them with a thin ſtratum of gold, a greater value than they really poſſeſs. Hence the origin of gilding.

In real gilding, the metal is either immediately covered with leaf gold, or an amalgam is formed of gold and quickſilver, with which the metal is rubbed over, and the quickſilver is then ſublimated by means of heat.

In gilding, a great deal depends on cleaning properly the ſurface of the metal to which the gilding is to be applied, becauſe the union of the gold with the metal becomes then more intimate. Silver, copper, braſs, and pinchbec, may be eaſily gilded, either by means of gold leaf, or of gold amalgamated with mercury; but iron and ſteel with difficulty; nor can ſo durable a gilding be given to them as to other metals by any proceſſes yet known. The cauſe of this is, that the ſurface of iron or ſteel during the proceſs of gilding cannot be kept thoroughly clean.

Gilding Iron or Steel with Gold Leaf.

In this proceſs the metal muſt be heated before the gold leaf can be applied, which expoſes it to the riſque of having its ſurface oxidated, beſides the danger of having the temper at the ſame time brought too low, when ſword-blades, daggers, and the like, are ſubjected to the proceſs.

Gilding with Amalgam and Nitrate of Mercury.

In gilding iron or ſteel by means of an amalgam, the difficulty and the danger of miſcarrying are ſtill greater; for, as the metal has no affinity for the mercury, an agent muſt be employed to diſpoſe the ſurface to receive the gilding. For this purpoſe a ſolution of mercury in nitrous acid (*aquafortis*), or what the workmen call quickſilver water, is applied to the parts intended to be gilded: the acid, by a ſtronger affinity, ſeizes on a portion of the iron, and depoſits in the place of it a thin coating of mercury, which will not refuſe a union afterwards with the gold amalgam that may be applied; but by this proceſs the ſurface of the metal is injured by the nitrous acid, and the union of the mercury is very ſlight, ſo that a bright and durable gilding cannot be obtained.

Gilding with Amalgam and Sulphate of Copper.

Sometimes a ſolution of ſulphate of copper (*blue vitriol*) is applied, with a camel's hair pencil, to the parts of the ſteel intended to be gilt. By a chemical action exactly ſimilar to what we have deſcribed as taking place when a ſolution of nitrate of mercury is employed, a thin coating of copper is precipitated on the metal. Copper having an affinity for mercury, a kind of union may by this means be effected between the amalgam and the iron or ſteel, as the caſe may be.

In whichever of theſe ways the amalgam be brought into union with the ſteel, the ſurface is injured by the action of the acid employed, and ſtill a heat ſufficient to volatilize the mercury muſt be afterwards employed. Moſt artiſts, therefore, follow the firſt proceſs, that of applying gold leaf, and burniſhing it down on the article while hot; which, though a laborious proceſs, is attended with leſs riſk of the ſurface being injured.

An improved Proceſs for gilding Iron or Steel.

This proceſs, which is leſs known among artiſts than it deſerves to be, may prove uſeful to thoſe who have occaſion to gild iron or ſteel. The firſt part of the proceſs conſiſts in pouring over a ſolution of gold in nitro-muriatic acid (*aqua regia*) about twice as much æther, which muſt be done with caution, and in a large veſſel. Theſe liquids muſt then be ſhaken together: as ſoon as the mixture is at reſt, the æther will be ſeen to ſeparate itſelf from the nitro-muriatic acid, and to float on the ſurface. The nitro-muriatic acid becomes more tranſparent, and the æther darker than they were before; the reaſon of which is, that the æther has taken the gold from the acid. The whole mixture is then to be poured into a glaſs funnel, the lower aperture of which is ſmall; but this aperture muſt not be opened till the fluids have completely ſeparated themſelves from each other. It is then to be opened; by which means the liquid which has taken the loweſt place by its greater gravity, viz. the nitro-muriatic acid, will run off; after which the aperture is to be ſhut, and the funnel will then be found to contain nothing but æther mixed with the gold; which is to be put into well-cloſed bottles, and preſerved for uſe. In order to gild iron or ſteel, the metal muſt be firſt well poliſhed with the fineſt emery, or rather with the fineſt *crocus martis* or *colcothar of vitriol* and common brandy. The auriferous æther is then to be applied with a ſmall bruſh; the æther ſoon evaporates, and the gold remains on the ſurface of the metal; the metal may then be put into the fire, and afterwards poliſhed. By means of this auriferous æther all kinds of figures may be delineated on iron by employing a pen or fine bruſh. It is in this manner, we believe, that the Sohlinger ſabre blades are gilded. As every artiſt has not an opportunity of procuring a recipe for the preparation of the nitro-muriatic acid and æther, we ſhall here give proceſſes for both.

Preparation of the Nitro-muriatic Acid.

In a ſufficient quantity of nitrous acid diſſolve as much ſal-ammoniac as it can take up in a cool place. Into this nitro-muriatic acid put the gold, after it has been reduced to fine powder by being filed or beat into thin leaves, and depoſit it in a warm place until it is diſſolved. This ſolution has a golden-yellow colour, and gives a purple tint to animal bodies.

Preparation of the Æther.

Put into a large retort one pound and a half of the ſtrongeſt ſpirit of wine, and pour gradually into it two pounds of the ſtrongeſt ſulphuric acid; ſhaking the retort a little each time the acid is poured in. The liquid will become hot, and the vapours, diſengaged with force, will diffuſe a penetrating and agreeable odour. When you have poured in all the ſulphuric acid, rinſe the neck of the retort by pouring in half a pound more alcohol; and having mixed the whole thoroughly, ſuffer it to ſtand for ſome time well cloſed up.

Then place the retort with the mixture in a ſand-bath, and adapt to it a capacious receiver, and make a fire under the retort. The heat however muſt be ſlow, and ſo moderate that the receiver may never be heated by it. The diſtillation muſt be continued until a ſulphurous odour, inſtead of the agreeable one, is obſerved at the mouth of the receiver. For this purpoſe the receiver may be now and then emptied, by which means the odour can be at all times obſerved.

When the diſtillation is ended you will have obtained an æther; which however always contains partly a little acidulous water, and partly ſome ſpirit of wine.

To obtain it perfectly pure, or to rectify it, put it again into a retort; and having added to it a little diſſolved alkali, in order to abſorb the acid, place it in a very gentle ſand-bath, and adapt to it a receiver. If the half of it be then driven over at a very gentle heat, you will obtain very pure æther.

What remains after the firſt diſtillation conſiſts, for the moſt part, of ſulphuric acid impregnated with ſulphurous gas and with carbonaceous matter furniſhed by the decompoſition of the alcohol.

This reſiduum may be again employed for the preparation of æther. For this purpoſe pour over it good ſpirit of wine, but always a third leſs than before, and proceed to diſtillation: if you then rectify, you will obtain good æther.

Another good Proceſs for Gilding.

Æther is proper, as has been above remarked, for taking up gold from the nitro-muriatic acid. Thoſe to whom this preparation of æther, for the purpoſe of gilding, appears too difficult, may uſe in its ſtead eſſential oils, ſuch as ſpirit of turpentine, oil of lavender, and the like; which will alſo take gold from its ſolution.

To prepare the Alcohol.

As it is ſometimes difficult to procure alcohol ready prepared, the following method of rectifying weak ſpirit of wine will be found very advantageous to artiſts:

Take well dried potaſh and pour over it ſpirit of wine: the latter will not unite with the potaſh, but the water which it may contain will be taken up by that alkali. The ſpirit of wine is then to be poured into another glaſs, and ſubjected to the ſame operation as before. This proceſs is to be repeated till it is obſerved that the potaſh is no longer very moiſt. Such ſpirit of wine is exceedingly ſtrong, but rendered a little impure by the potaſh, as will appear from its yellowiſh colour. It muſt therefore be poured into a retort, having a receiver adapted to it, and diſtilled to a fifth part over a ſlow heat. What comes over is alcohol.

Cold Gilding of Silver.

Beſides the above methods of gilding, there is alſo a cold gilding of ſilver, which can be ſpeedily done, and with little trouble.

Diſſolve gold in the nitro-muriatic acid, and dip ſome linen rags in the ſolution; then burn them, and carefully preſerve the aſhes, which will be very black, and heavier than common.

mon. Rub theſe aſhes on the ſurface of the ſilver either with the fingers or a piece of leather or of cork, by which means the fine particles of gold they contain will be left on the ſurface of the metal. If the ſilver be then waſhed, ſo as to remove the remaining part of the aſhes, the ſilver will have ſcarcely any appearance of gilding; but on being burniſhed it will aſſume a beautiful gold colour. This method of gilding is very eaſy, and requires very little gold.

Gilding of Braſs.

Fine inſtruments of braſs, in order that their ſurface may be kept longer clean, may be gilded in the following manner: Provide a ſaturated ſolution of gold, and, having evaporated it to the conſiſtence of oil, ſuffer it to ſhoot into cryſtals. Theſe cryſtals muſt then be diſſolved in pure water, and the articles to be gilded being immerſed in it are then to be waſhed in pure water, and afterwards burniſhed. This proceſs may be repeated ſeveral times till the articles have been well gilt. A ſolution of gold cryſtals is preferred to a mere ſolution of gold, becauſe in the latter there is always a portion of free acid, which will not fail to exerciſe more or leſs action on the ſurface of the braſs or copper, and injure its poliſh.

Lackering of Braſs.

Various articles made of braſs have ſometimes an appearance as if they were gilded. This appearance, however, is produced by means of a ſolution of gum-lac in ſpirit of wine, with which they are rubbed over. As long as the lac laſts, they retain their ſplendour. Theſe articles, however, are attended with this inconvenience, that they muſt never be cleaned with a ſtrong bruſh, or ſcoured with chalk and the like, but only be wiped with a ſoft rag; for as ſoon as the lac is rubbed off they loſe their brilliancy. A varniſh of this kind may be prepared in the following manner:

Diſſolve two ounces of very pure and fine gum-lac in 48 ounces of alcohol, and place the ſolution in a ſand-bath expoſed to a moderate heat. To prevent the too abundant evaporation of the ſpirit of wine, as well as the burſting of the glaſs, a piece of bladder ought to be bound over the latter, and a few holes made in it with a needle. In another glaſs diſſolve, in the ſame quantity of ſpirit of wine, an ounce of dragon's blood in grains. When both the ſolutions are completed, mix them together; then put three grains of yellow wood into it, and ſuffer it to remain there twelve hours in a moderate heat: after which ſtrain the liquor through filtering paper, and preſerve it for uſe in a clean glaſs bottle. To give this lac-varniſh a high gold colour, yellow wood is preferable to every other ſubſtance. If the varniſh is intended to be pale, and not to change the colour of the braſs, the yellow wood may be omitted; but if a ſtronger colour be required, a half more of the yellow wood may be added.

XXIV. *Letter from Profeſſor* PICTET, *F.R.S. Profeſſor of Natural Philoſophy at Geneva.*

To Mr. Tilloch.

SIR, Paris, October 23, 1801.

DURING my ſtay here, on my return from England, I have had the ſatisfaction of meeting often the celebrated Italian profeſſor Volta, who, in company with his friend Brugnatelli, profeſſor of chemiſtry at Pavia, has undertaken a philoſophical journey to this metropolis to give and to collect information. I have been repeatedly a witneſs to a moſt ingenious and intereſting ſeries of experiments, by which profeſſor Volta demonſtrates the evident identity between what has been called the *galvanic fluid* and common electricity. A ſhort expoſition of the leading facts will not, perhaps, be unacceptable to your philoſophical readers.

He begins by ſhowing, with the help of his conductor and a common electrometer made with two hanging bits of ſtraw, a new and fundamental fact in electrics, viz. that different metals (and even other ſubſtances) mutually impart to one another, *by a mere dry contact*, a certain quantity of electricity: after this exchange, made rather by a kind of impulſe than by affinity, they are found to poſſeſs oppoſite ſtates of electricity, eaſily demonſtrable by the common teſt with ſealing-wax.

He ſhows afterwards, that if a ſyſtem of two different metals be diſpoſed in the following order—copper, zinc, copper—without any intermediate conducting liquid, no change of the electric ſtate takes place in that caſe; becauſe the influence of the copper on zinc on one ſide, is balanced by a ſimilar and contrary influence on the other. But if a piece of wet paper be put between, then the electrical equilibrium is immediately deſtroyed by the contact, and the phænomena appear.

The better the conducting liquid happens to be, the greater is the effect. Thus brine or acidulated water act more ſtrongly than pure water, *merely as better conductors.* This fact he alſo proves by direct experiments.

On theſe three fundamental facts he then builds his whole fabric. Let the apparatus be a pile, a ſeries of cups, or a trough, his theoretical poſitions anſwer equally well. He explains, among many curious phænomena, why the Voltaïc pile charges a large Leyden jar, by a ſingle inſtantaneous contact, much more than the electrical machine in the ſame circumſtance.

He thus transfers, from a pile of 100 or 200 couples, to a very large Leyden jar, and even to a battery, by a ſingle contact, which laſts not above 1-10th of a ſecond, the ſame degree of electrical charge which the electrometer ſhows to exiſt in the pile, and the faculty of giving a ſhock equally ſtrong. Thoſe experiments require the moſt perfect continuity in the conductors; metallic *chains* muſt therefore be excluded from the circuit.

The frogs and other animals put in motion after death by what has been hitherto called the *galvanic influence,* are in his theory but mere electrometers. He ſhows, in particular, that a half frog, diſpoſed in the common way, is almoſt indefinitely ſuſceptible of muſcular contractions by merely inverting alternately its poſition relative to the direction of the electrical current: that is, when blunted, for inſtance, by a number of diſcharges paſſing from the right leg to the left, it recovers its whole ſuſceptibility if it be but half turned round, ſo as to reverſe the courſe the fluid takes; and this repeatedly for a long while, till bordering on putrefaction.

A commiſſion choſen among the ableſt electricians in the National Inſtitute, has been named to examine both the theory of the learned profeſſor and the experiments on which it is grounded. He intends to collect ſoon, in one ſingle performance, the moſt eſſential facts and inferences he has publiſhed at different times on the ſubject; and in the mean time my friend Mr. De la Metherie prepares for the *Journal de Phyſique* an account more circumſtantial than the ſlight ſketch I am now forwarding to you, rather fit, I confeſs, to excite than to gratify your curioſity.

I am, &c.

M. A. Pictet.

XXV. *Letter to the Editor of the Philoſophical Magazine.*

Reſpected Friend,

THE ſubjoined paper has appeared to me, on peruſal, ſufficiently intereſting to deſerve publication in the Philoſophical Magazine. It is another proof (which I was not poſſeſſed of at the time my eſſay on the ſame ſubject appeared) of the attention which this queſtion had obtained on the continent; and as, notwithſtanding a conſiderable ſimilarity in the introductory part, our inquiries appear to have been directed to diſtinct though cloſely-connected branches of the ſubject, each of theſe papers may prove to the reader a uſeful commentary on the other. The inſertion of it will therefore be acceptable to

Thy Friend,

Plaiſtow, 16th of the Tenth Month, 1801. L. Howard.

On the Influence which the Sun has on the State of the Barometer.

By J. J. Hemmer *.

WHEN philoſophy, in modern times, emerged from that lamentable ſtate of darkneſs in which it was for ſo many years involved, and philoſophers employed the more rational and certain way of obſervation and experience for examining the nature of things, all the aſſertions and opinions of the antients reſpecting the influence of the heavenly bodies were ſubjected to more accurate inveſtigation. Some, as being contrary to experience, were totally rejected; others were retained, and placed beyond all doubt, as they evidently coincided with nature; and others were claſſed among uncertainties, as experience furniſhed nothing deciſive for or againſt them. Among the number of the latter, according to moſt modern philoſophers, is the opinion that a part of the wonderful variations which we obſerve in the barometer are to be aſcribed to the effects of the ſun and moon, and other heavenly bodies. This opinion, indeed, after a long ſeries of experiments and reſearches, appeared to Paſcal, Garcin, Wallis, Halley, De la Hire, Mariotte, Woodward, Leibnitz, Mairon, Bernoulli, Muſchenbroek, De Luc, &c. ſo little ſupported by proofs drawn from nature, that they did not think proper to mention it. This, however, is much to be wondered at in regard to De Luc, as he ſays in his work on the atmoſphere, that it is attracted with more or leſs force by the planets, according to their different diſtances.

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* From the *Tranſactions of the Electoral Academy of Sciences at Erfurt*, vol. vi.

Some celebrated philoſophers, however, of the preſent day have confidently believed, that if the variations of the barometer were carefully compared with the different poſitions and diſtances of the heavenly bodies, and particularly of the ſun and moon, it would be found that many conſiderable variations of the ſtate of the mercury in the Torricellian tube are effected by them. For, as the flux and reflux of the ſea depend on the ſun and moon, it cannot be doubted that in the atmoſphere of the earth, which is much lighter than the water of the ſea, movements of the ſame kind, which have an influence on the barometer, muſt take place. The firſt experiments on this ſubject were made about fifteen years ago by the celebrated Lambert, who employed for that purpoſe eleven years obſervations of the barometer made by Doppelmayer of Nuremberg, and compared them with the apogeum and perigeum of the moon. He was not able, however, to obtain any certain reſult, and conſidered the period of the obſervations as too ſhort.

Toaldo, who was furniſhed with much more abundant materials, reſumed the ſubject, and by means of the forty years obſervations of the Marquis de Poleni and ſon, with eight years of his own, had a ſeries of forty-eight years obſervations. After a laborious compariſon of theſe with the apſides, ſyzigies, and quadratures of the moon, and with the paſſage of the moon and ſun through the ſigns of the zodiac, he thought he diſcovered undoubted proofs of a perceptible influence of the moon and ſun on the barometer. M. Friſi of Milan endeavoured to controvert this aſſertion, which had been the reſult of ſo much labour, and maintained that the variations of our atmoſphere have no ſimilitude to the flux and reflux of the ſea, and can admit of no compariſon, as the latter is regular and invariable, the former only temporary. The flux and reflux of the ſea, in regard to time and duration, are ſo certain, that there are tables of them for all the principal ſea-ports, which never err; whereas the variations of the atmoſphere are at the ſame time in different places, and in the ſame place at different times, ſo great, that where it rains at one period for three or four months it is ſometimes as long without rain. It ſufficiently appears at preſent, from mathematical calculations, that during the paſſage of the moon or ſun, from the horizon to the meridian, the daily height of the barometer cannot be changed more than $\frac{1}{5}$ of a line, Paris meaſure, by the effect of the former, and about $\frac{1}{4}$ by the effect of the latter.

To many, however, Friſi's reaſons did not appear ſo concluſive as to make them give over their reſearches on this ſubject;

jeƈt; and in particular, moſt of the members of our meteorological ſociety, among whom M. Steiglehner, Planer, and Chiminello, have taken great pains. The firſt ſays, that he found, by ſeveral comparative obſervations, that the greateſt fall of the barometer does not happen in very remote places at the ſame time, but that it is earlier towards the weſt, and later towards the eaſt, and that the difference of the time is nearly equal to the difference of the meridians of the places; an aſſertion which indeed deſerves to be more accurately examined.

M. Planer obſerved the barometer for a whole year, ſix times every day, or every fourth hour of the natural day, at two, ſix, and ten in the morning, and at the ſame hours after noon; and found, in general, that the barometer, between ten in the morning and two in the afternoon, and between ten at night and two in the morning, was leſs in its riſing, and greater in its fall; and that the contrary was the caſe between the hours of ſix and ten in the evening and morning. M. Chiminello obſerved the barometer twenty-two times a day for three years, but he left a chaſm in the night which he ſupplied by calculation. The principal poſitions which he thence deduced, are, that the barometer falls towards noon, as well as towards midnight *.

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* In examining a twelve-months' Regiſter, kept by —— Dunbar, Eſq. near the banks of the Miſſiſippi, in N. lat. 31° 28′, and long. 91° 30′ W. of Greenwich, which, for the greater facility of compariſon, I have laid down, with others, on a ſcale in the manner I have heretofore exhibited, there occurs a remarkable inſtance of a diurnal variation. For the ſpace of about four days before, and ſix days after the ſummer ſolſtice, the barometer regularly riſes from about 9 P. M. to about 6 A. M. then falls till the return of the former hour in the evening, then riſes again as before, &c. in alternate periods. In the firſt four days the direƈtion is *aſcending*, and the elevation of a line drawn through the mean is about $\frac{6}{100}$ of an inch. In the latter ſix days the mean line is perfeƈtly horizontal, the elevation each night amounting to $\frac{2}{100}$ and the depreſſion each day to the ſame, but occupying double time. The times above given are thoſe at which the obſervations were made, but it is probable that the maximum and minimum each day correſponded rather with the times of ſun-riſe and ſun-ſet. The firſt period of four days was dry, with a temperature of 92° in the middle of the day. This ended in a thunder ſtorm, on the 21ſt of the month, with 0,82 inches of rain. The barometer, after this, roſe $\frac{4}{100}$ in the night, then remained moſtly ſtationary, with cloudy weather, until the evening of the 26th, beginning a ſecond period of ſix days, during which briſk winds at S. S. E. and S. W. prevailed with rain every afternoon or evening, amounting, in all, to 1,7 inches. Temperature 85°. The whole occupied juſt the ſpace between full and new moon; and there are traces of the operation of this periodical influence in other parts of the Regiſter.

I ſhall not preſume, at preſent, to aſcribe this variation entirely to planetary influence; but the facts are worthy examination in that reſpect. L. H.

The obſervations of theſe three men afford ſome indications of the influence which the heavenly bodies have on the barometer, and to which M. Planer aſcribes part of the effects: but he is far from proving this; for the number of the places the obſervations were made at, which M. Steiglehner compared, were not above three, and conſequently too few to determine the queſtion. The caſe is the ſame with the ſix obſervations of M. Planer, made daily, as has been obſerved by M. Chiminello. And might not what is aſcribed to the period of four hours have happened in the laſt hour, or the laſt half-hour? Chiminello's experiments are of more importance, but for want of night obſervations they are ſtill defective and imperfect.

In this reſpect, however, I was more fortunate, as I had an eaſy and ſure method of following all the variations of the barometer, and even the ſmalleſt, by night as well as by day. I had in my poſſeſſion, at that time, a barometer, belonging to the Electoral Phyſical Muſeum, invented lately by M. Changeux, and called a barometrograph, which being connected with a clock ſhows the ſtate of the mercury every four minutes, by means of a point impreſſed on a moveable plate. I began on the 21ſt of May 1785 to make uſe of this excellent inſtrument, and it appeared to me, by repeated obſervation, that a continual variation took place towards noon as well as towards midnight. By more accurate obſervation, I found that this variation in the mercury conſiſts, at leaſt, in an inclination to fall. But that I might be more certain in treating a queſtion of ſo much importance, I reſolved to compare all the obſervations from the firſt day to the end of the year, which are printed in five volumes of our Ephemerides. In that time I found that the paſſage of the ſun over the meridian by day and by night had taken place 446 times, and conſequently as many variations of the barometer were to be conſidered. From the moſt accurate compariſon, the following three rules preſented themſelves:

1ſt, When the ſun paſſes the meridian, the barometer, if in the act of falling, continues to fall, and the falling is accelerated.

2d, When the ſun paſſes the meridian, the barometer, if in the act of riſing, falls, or becomes ſtationary, or riſes more ſlowly.

3d, When the ſun paſſes the meridian, the barometer, which is ſtationary, falls, if it has not riſen before or after being ſtationary; in which caſe it uſually becomes ſtationary during the ſun's paſſage.

The firſt rule had only one exception, the ſecond none, and the third ſix. The laſt were all in the night-time except one. On this occaſion, the barometer during the ſun's paſſage over the meridian ſtood ſtill, though it fell before and after. When theſe ſeven exceptions are deducted from the 446 variations there remain 439, which accord with the rules. When I ſay, however, that theſe variations took place during the ſun's paſſage over the meridian, I do not mean by that expreſſion the very moment when the ſun is exactly in the meridian, but the whole period when he is near it.

All theſe variations took place between eleven o'clock and half after one, conſequently within the ſpace of an hour and a half; except ſix, five of which happened half an hour or a quarter before eleven, and one about an hour after noon; which, indeed, muſt be aſcribed to a particular cauſe, which either haſtened or retarded the effect of the general cauſe. Of the above-mentioned 439 variations, 54 took place after twelve o'clock, and all the reſt before one.

As it is therefore certain that the barometer during the paſſage of the ſun over the meridian is always inclined to fall, it cannot be doubted that the cauſe of ſo invariable a phænomenon, which follows ſo remarkable laws, and recurs at the ſame time under the like circumſtances, muſt be conſtant and invariable. It is at preſent well known that the principal atmoſpheric cauſe of the falling of the barometer is heat and evaporation. But it appears, on cloſer examination, that neither of theſe can produce the phænomenon in queſtion. What M. De Luc, however, ſays *, that the leaſt height of the barometer, when no particular cauſe intervenes, always takes place about the hotteſt part of the day, which in every ſeaſon is about ¾ of the artificial day, might lead to a ſuſpicion that the falling of the barometer about noon proceeds from the heat. For, as there is a conſiderable increaſe of heat about noon, a perceptible falling of the mercury about the ſame time muſt be connected with it, until, in the above-mentioned part of the day, the mercury reaches its loweſt ſtate, and the heat attains to its maximum. But if we admit that the greateſt heat commonly takes place about the ¾ of the day, experience forbids us to aſcribe to the ſame period the greateſt falling of the mercury. I have therefore conſtructed a table in which the loweſt ſtate of the mercury for each day of the month, between noon and the evening, or night, is given from the above-mentioned barometrographic

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* *Recherches ſur les Modifications de l'Atmoſphere*, vol. ii. p. 94.

obſervations*. This ſtate of the barometer takes place when the mercury ceaſes to fall and afterwards begins to riſe, or before riſing is ſtationary. When it does not riſe after noon, but ſinks continually or by intervals, the loweſt ſtate after noon muſt be at midnight. The caſe is the ſame when it firſt riſes after noon, and afterwards falls towards the end of the natural day. But when it continually riſes after noon, though this movement may be interrupted by its once becoming ſtationary, the loweſt ſtate when it falls early, or when this does not happen, is to be conſidered as taking place about twelve at noon. When in this caſe a frequent falling takes place in the morning, I have obſerved that it was neareſt noon and the $\frac{3}{4}$ of the day: when the quickſilver roſe and fell ſeveral times between noon and evening, I marked ſeveral hours, without further examining at which the ſtate of the barometer was loweſt.

Now it appears from theſe tables, that in 222 days the loweſt ſtate of the barometer correſponded only ten times with the $\frac{3}{4}$ of the day; on the other hand, it deviated from that period ten times about half an hour, at other times a whole hour, and often half a day; and conſequently De Luc's rule, at leaſt according to my obſervations, is not correct.

We may therefore aſk thoſe who aſcribe the falling of the barometer to heat, Why ſhould this cauſe be ſo cloſely confined to the period between 11 in the morning and the middle of the firſt hour after midnight? Why does it not take place earlier or later, when the mercury riſes continually the whole day, or ſeveral days either fall a little towards noon, or at leaſt become ſtationary or riſe ſlowly? and, Why does not this rather take place ſeveral hours after noon, the time at which the heat generally is the greateſt?

But that heat is not the cauſe of this phænomenon is proved by the falling of the mercury at midnight; which is no leſs common and certain than the falling at noon: on account of the cold, it ought then rather to riſe or to fall.

We may aſſert the ſame thing of vapour as of heat, as theſe have the cloſeſt connection with each other. As a certain accumulation of vapours diſſolved in the atmoſphere makes the air lighter, and obliges the barometer to fall, the power with which it acts on the inſtrument muſt be in proportion to its quantity. When it obliges the barometer, therefore, to fall towards noon, why does the latter ſo often riſe afterwards? What then leſſens the quantity of theſe vapours when the heat, which by its nature promotes their ſolution, ſtill increaſes, or at any rate does not become leſs? How does it happen that the barometer falls in the night? as about that time the vapours are, for the moſt part, precipitated from the atmoſphere. Why do they exerciſe the greateſt force exactly at midnight? and, By what law of nature are they more connected with that than with any other period?

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* This table has been omitted in this tranſlation, becauſe the reſults here given are ſufficient.

It may here evidently be ſeen, that the conſtant falling of the barometer about noon and midnight can be occaſioned neither by heat nor by the effect of vapours.

Beſides theſe two cauſes, and the winds, we are acquainted with no others in our atmoſphere which can make the barometer to fall. But as the wind often becomes calm about noon and midnight, or, when it blows, is of ſuch a nature that it does not make the column of air which preſſes on the barometer lighter, it is evident that the falling of the mercury about theſe times cannot ariſe from the wind.

What then remains but to ſeek without our atmoſphere for the cauſe of this phænomenon? It is evident that it is to be found in the ſun; for, as by its attraction it moves the water of the ocean, it muſt exerciſe a ſtill ſtronger action on the column of air; and from what is known reſpecting the flux and reflux of the ſea, the whole phænomenon may be eaſily explained. In the firſt place, as the atmoſpheric flux and reflux themſelves muſt depend on the ſun's paſſage over the meridian, it muſt alſo take place both at the diurnal and the nocturnal paſſage of the ſun; becauſe the flux and reflux of the atmoſphere, like thoſe of the ſea, muſt happen at the ſame time on oppoſite ſides of the terreſtrial hemiſphere. In the laſt place, the atmoſpheric flux which produces the falling of our barometer is only an effect of the flux which takes place between the tropics, where the air during the paſſage of the ſun aſcends, and therefore proceeds thence from us, as happens in all the tides of the ſea which lie beyond theſe circles. That theſe tides, which ariſe by communication, are tranſmitted more ſpeedily and by a longer duration in the atmoſphere than in the ocean, may be concluded from this circumſtance, that the particles of the air have leſs gravity and leſs mutual adheſion.

XXVI. *Letter from* L. W. Dillwyn, *Eſq. to* W. H. Pepys *jun. Eſq. reſpecting the Effects of the Oxymuriatic Acid on the Growth of Plants*[*].

My dear Friend, Higham Lodge, Sept. 7, 1801.

THE few experiments I made on the effects of the oxygenated muriatic acid on the growth of plants, I conceive to be too inſignificant to merit much attention: however, at thy deſire, I here relate them, and give thee the few thoughts that have occurred reſpecting them.

From what M. Humboldt aſſerts reſpecting raiſing ſeeds ſo very rapidly by means of the oxymuriatic acid, the queſtion occurred to me, whether it might not be ſucceſsfully uſed for raiſing early crops of muſtard and creſs, radiſhes, &c. and therefore early in March I ſowed two patches of each (as well as of ſome other plants) in a ſheltered ſituation in the open air, and kept one of each conſtantly moiſt in the common way, and the other with a mixture of one large tea-ſpoonful of oxymuriatic acid to a pint of water; and in no inſtance did there appear to be any difference with reſpect to the time of germination.

I then tried the ſame experiment on two pots of balſams in a hot-bed, and alſo on ſome coxcombs. Thoſe balſams moiſtened with the diluted oxymuriatic acid all came up in 90 hours, whilſt none of the others appeared till near 30 hours after; and the combs came up about in the ſame proportion. I ſhall next give the particulars of an experiment I made in warmer weather in the open air. On the 2d of May I ſet four different lots of garden beans, with three in each lot, in the following manner:

Lot 1. I ſoaked in 24 drops of oxymuriatic acid, diluted with two large tea-ſpoonfuls of water, for ſeven hours, and, immediately before I took them out, added ſix drops of acid more.

Lot 2. I ſoaked in twelve drops of acid and two ſpoonfuls of water for the ſame time, and, immediately before I took them out, added three drops more.

Lot 3. I ſoaked for the ſame time in plain water.

Lot 4. were not ſoaked at all.

Theſe were then all planted at the ſame depth in a ſheltered border, and lots 1, 2, and 3, kept conſtantly moiſt with oxymuriatic acid diluted with water in the proportion of one large tea-ſpoonful to a pint.

On May 13, one of No. 2 came up, and on the morning of the 14th one each of Nos. 1 and 3, of which No. 1 was forwardeſt; and in the courſe of the day the others of lots 1, 2, and 3, appeared. On the 16th they all looked finely; but thoſe of lot 1 were largeſt, and thoſe of lot 2 larger than lot 3: thoſe of lot 4, which were kept watered with plain water, did not come up till the 18th, or perhaps one of them on the evening of the 17th. On June 5 they were all nearly fully grown; but thoſe of No. 1 were conſiderably ſtunted, and thoſe of lot 2 ſomewhat ſo, and neither of them were ſo fine as thoſe of lots 3 and 4.

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[*] Communicated by Mr. Pepys.

In the latter end of ſpring and beginning of ſummer I tried the ſame experiments on ſome other plants, but, from not being conſtantly at home, could not make them with ſufficient accuracy; however, enough ſo to determine that thoſe moiſtened with dilute oxymuriatic acid came up firſt.

In the experiment on the beans, we find that thoſe ſoaked and moiſtened with the dilute acid came up ſome days before thoſe that were only watered in the common way, and that thoſe which had the moſt acid at firſt were the forwardeſt, and apparently moſt flouriſhing plants, though afterwards they the moſt rapidly declined; and therefore I conclude (though more experiments are wanting on this ſubject poſitively to determine), 1ſt, That the diluted oxymuriatic acid *only* in a certain temperature hath the effect of promoting an earlier germination of ſeeds than would otherwiſe take place: and, 2d, From the plants at firſt growing more rapidly than thoſe watered in the common way, but afterwards declining ſo as to be ſurpaſſed by them, that the oxymuriatic acid poſſeſſes no nutritive quality, but acts merely as a ſtimulus, and therefore cannot be applied advantageouſly to the purpoſes of horticulture. I am very truly and reſpectfully

Thy ſincere Friend,
L. W. DILLWYN.

XXVII. *Obſervations on the Queſtion, whether mixed Metals can be diſtinguiſhed by the Smell.* By M. BŒTTIGER*.

WERNER†, Wiedemann, and other chemiſts, place ſmell among the external characters of foſſils. They divide minerals into odorous and inodorous, and make the firſt claſs to conſiſt of ſwine's ſtone, ſome other earthy bitumens, and ſulphureous and arſenical pyrites, which when rubbed or

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[*] This paper firſt appeared in the *German Mercury*, publiſhed by Wieland, March 1800: it was afterwards inſerted in the *Magaſin Encyclopédique*, No. 10, an. 10, from which we have tranſlated it.

[†] Sur les Caractères extérieurs des Foſſiles, § 204, p. 2080. Lenz: tom. i. p. 19.

ſtruck exhale a certain odour. The antients were acquainted with the ſmell of the ſemi-metals, experiments on which were made by Boyle. But I do not find in any of the mineralogical works of the moderns that the purity or alloy of the noble metals, ſuch as gold and ſilver, can be aſcertained merely from the external character of ſmell. Among the antients, however, ſmell was a principal character of metals. The fineſt compoſitions were diſtinguiſhed merely by their odour. Arrian on Epictetus * ſays expreſsly that an expert money-changer (banker) muſt neceſſarily have the ſenſe of ſmelling in an exquiſite degree. "Behold," ſays Epictetus, "in money affairs, when our intereſt is concerned, the art which has been invented, and the means which the changer employs to try the money. He tries them by the ſight, by the touch, by the ſmell, and even by the hearing †; for he throws

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* I. 26. p. 110. edit. of Schweighauſer.

† And why not by the taſte? to go through the whole circle of the organs of ſenſe. This method was alſo known to the antients. The uſe of the touchſtone, *lapis Lydius* (ſee Theocritus, xii. 36; and Pliny, xxxiii. 8. with obſervations by M. Schneider, *Analecta ad Hiſtoriam Rei Metallicæ*, p. 3.), was, however, the moſt common method of trying gold, which when it had undergone this teſt was called *obryzum*. Pliny ſpeaks alſo, in the place above quoted, of trials by fuſion. There is a curious paſſage in Herodotus, vii. 10. reſpecting the true meaning of which mineralogiſts, in my opinion, have not yet agreed. Artabanus ſaid to Xerxes before the Perſians aſſembled, that pure gold could be diſtinguiſhed by rubbing it againſt other gold. Weſſeling, p. 312. 30. conſiders this idea ſo abſurd, that inſtead of *to rub* (παρατριψωμεν) he prefers reading *to compare* (συγκρινηται). Larcher, in his tranſlation of Herodotus, vol. v. p. 272, adopts, in part, the opinion of Weſſeling on this paſſage: he ſays the word in the Greek text is παρατριψωμεν. But it is not by rubbing gold againſt gold that we can diſtinguiſh which is the beſt; I do not therefore heſitate to adopt the reading of Sancroft's manuſcript, επεαν δε ἑτερῳ συγκρινηται, which indicates the only manner of aſcertaining pure gold before the uſe of the touchſtone was known. I confeſs, however, that παρατριψωμεν has always appeared to me preferable, becauſe συγκρινηται has the appearance of an interpolation, and may have been ſubſtituted by ſome one who did not underſtand the other word. My opinion is confirmed by C. Girod-Chantram, who, when conſulted on this paſſage, declared that it might be retained without any change, becauſe, pure gold being ſofter than that alloyed with copper, the leaſt pure metal muſt make an impreſſion on the other, and thus afford the means of aſcertaining it. The proof of this aſſertion was eſtabliſhed at the time when it was propoſed to the National Convention to coin new money in France without any alloy. The advantages of this method were found to be counterbalanced by ſome inconveniences, one of which was, that friction is more deſtructive to pieces of pure metal than to thoſe the hardneſs of which is increaſed by alloy—*Rapport par Loyſel*, Sept. 15, 1793. The National Convention charged the Academy of Sciences to make experiments, which fully confirmed the effect in regard to continual friction on pure and alloyed metals. "It thence reſulted," ſay the commiſſioners in their report, "that the loſs experienced

throws the money on the ground and remarks the ſound. He is not even contented with ſounding it once or twice, and the great attention he employs renders him a muſician." However, as the antients generally employed the pureſt gold for coining money[*], it appears that the proof by ſmelling did not relate to pure gold and ſilver, but to the ignoble metals which might have been mixed with them. For all the paſſages of antient authors which ſpeak of the ſmell of metals[†] allude to an artificial compoſition which could be diſtinguiſhed by its odour. It appears that it was in particular the compoſition ſo fooliſhly ſought after by the Romans, to which, by way of preference, they gave the name of Corinthian braſs, and which they made uſe of for drinking-cups and their moſt valuable table ſervices. We muſt no doubt rank among this number the pretended *vaſa Batiaca* which Alexander the Great found among the effects of Darius, and which being compoſed of bronze brought from India could not be diſtinguiſhed from gold but by the ſmell[‡].

I am acquainted with two other paſſages which this remark may ſerve to explain. Martial, in the 60th epigram of his ninth book, deſcribes a ſwaggering fellow, who having a great deſire to purchaſe, but an empty purſe, haraſſed the toymen of the Campus Martius or Septum at Rome by always cheapening and never buying. The poet, beſides other things, ſays of him, "He conſults his noſe reſpecting the ſmell of Corinthian braſs[§]."

Cicero, in his Paradoxes, ſpeaks of the connoiſſeurs of his time who purchaſed Corinthian bronzes with incredible avidity, and in examining them employed all their knowledge in the arts. He exhibits one of theſe haughty Romans in the attitude of examining with great earneſtneſs a night-chair of Corinthian braſs[||]; for night-chairs were made of the moſt precious metals in thoſe periods, when gold was

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perienced by pieces of fine ſilver in circulation compared with that of alloyed ſilver would be nearly in the ratio of 3 to 2; and that of pure gold compared with alloyed gold would be as 7 to 3."—*Rapport de Leyſel*, p. 19.

[*] At the commencement of the 17th century, Louis Savot explained exceedingly well the metallurgic part of numiſmatics in his *Diſcours ſur les Médailles antiques*, inſerted in the 11th volume of Grævius's *Theſaurus*. See Eckel.

[†] *Doctrina num. vet.* tom. i. p. 22. See Caſaubon on Suetonius in the Life of Veſpaſian, c. 23; Beckman on Ariſtot. Mirabil. auſcult. c. l. p. 99, 100; and his Hiſtory of Inventions, vol. iii.

[‡] Ariſtot. Mirabil. auſcult. c. l. p. 97.

[§] Conſuluit nares, an olerent æra Corinthon.

[||] *Parad.* v. ii. Si L. Mummius aliquem iſtorum videret matellionem Corinthium cupidiſſime tractantem.

ſometimes cheaper than ſilver [*], and when Corinthian braſs ſold at a higher price than either. The wit of this paſſage is entirely loſt, if we do not ſuppoſe that the connoiſſeur examined the value of the bronze and of the veſſel, by holding his noſe over it.

This circumſtance, on various accounts, deſerves to be carefully examined by our metallurgiſts. The firſt queſtion would be, to know whether copper, mixed with other metals, can really be diſtinguiſhed by the ſmell. A late French traveller aſcribes to the inhabitants of one of the Kamtſchatdale iſles ſo acute a ſmell, that they were able, he ſays, to diſtinguiſh by it immediately copper alloyed with gold [†]. The ſecond would be, to know the nature of this alloy.

The queſtion reſpecting the manner in which the Corinthian braſs was mixed and fuſed with gold or ſilver, has never yet been finally determined. Wiegleb [‡], and other chemiſts, made experiments, by fuſing ſome pieces of antient bronze; but even if they could ſhow with preciſion the manner of mixing them, their experiments would ſtill be doubtful, becauſe the antients were acquainted with ſeveral bronze mixtures [§]; and conſequently, when they fuſed antient bronze, they were not certain of its being Corinthian braſs.

Addition by the French Tranſlator.

Having thought it of importance to give particular intereſt to this paper, by ſubmitting it to ſome celebrated metallurgiſt at Paris, I begged C. Gillet Laumont to communicate to me his ideas on the queſtion propoſed by M. Bœttiger: "Whether copper, mixed with other metals, can be diſtinguiſhed by the ſmell, and to determine the nature of that mixture." He was ſo obliging as to give me the following anſwer:

"Several metals have a ſmell which is peculiar to them: that of *iron*, *lead*, *tin*, and in particular *copper* [||], may be diſtinguiſhed; and there can be no doubt that alloys compoſed of theſe metals muſt emit different odours. But the ſenſe of ſmelling among mankind in a civilized ſtate being leſs perfect and leſs conſtant, there are certainly many emanations which man cannot catch, or which would require a particular ſtudy which it is ſeldom his intereſt to apply to.

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[*] *Plin.* lib. xxxiii. 12. See Caylus *Recueil d'Antiquités*, vol. ii. p. 309.

[†] See Croſet *Nouveau Voyage à la Mer du Sud.* Paris, 1783, p. 258.

[‡] Acta Academiæ Moguntinæ, 1777, p. 50.

[§] For example, *orichalcum*, *electrum*, &c. There ſtill exiſt a great many medals of *electrum*.

[||] It is often neceſſary, in order to call forth their odour, to rub them, or at leaſt to examine them at a degree of heat equal to that of the human body. Care muſt be taken, in particular, to leave a ſufficient interval between each experiment, that the organs of ſmell may be entirely freed from the ſenſations produced by the preceding.

It is poſſible, alſo, that ſome perſons, highly favoured by nature, might acquire, by long habit, an exact knowledge of the quantity of copper that exiſts in any alloy; but it is certain that theſe perſons could not communicate to others at a diſtance, or perhaps even preſent, the kind of ſenſations which they experience, and their different modifications, according to the nature and proportion of the mixtures.

The means of diſtinguiſhing alloys, and the proportion of alloys, by the help of ſmell, appear then to me to form a ſecondary character, which ought not to be neglected, and which, in certain individuals, may, by habit, be brought to perfection, but which, as it cannot be eaſily communicated, can never become an eſſential and comparative character.

It is poſſible that, in the time of Alexander, among the means eaſily put in practice, that of ſmell was the ſureſt for diſtinguiſhing the Indian vaſes of bronze from thoſe of gold.

In regard to the paſſage of Herodotus, where it is ſaid "that pure gold was diſtinguiſhed by rubbing it againſt other gold," it appears to me that the word παρατρίψωμεν muſt be retained, but the word *againſt* ought to be changed into *by the ſide of*. The paſſage then becomes clear, and only ſuppoſes the employment of a hard body proper for retaining the traces of theſe metals. This explanation alludes to that kind of proof ſtill practiſed by our goldſmiths, with plates of metals alloyed in known proportions, which are called teſt plates; but to be certain of their aſſay, they make uſe of a touch-ſtone; that is to ſay, a common black ſtone, not ſuſceptible of being attacked by the acids which they pour upon it, to aſcertain the degree of alteration in the traces left by the alloys.

XXVIII. *Some Obſervations on the Agitations of the Lake of Derwent Water, and its floating Iſlands. By a Correſpondent.*

To the Editor of the Philoſophical Magazine.

Keſwick, October 7, 1801.

PASSING through this place with little time to ſpare from purſuits not philoſophical, I can only obtain imperfect information about ſome curious phænomena hitherto but little noticed; by ſome as little regarded as tales of enchantment, and as little underſtood by others as the moving of the waters of the pool of Betheſda.—I mean the agitation of the lake of Derwent water, and its floating iſlands. Pennant ſays, the water of Derwent water is ſubject to violent agitations, and often without any apparent cauſe, as was the caſe this day: the weather was calm, yet the waves ran a great height, and the boat was toſſed violently with what the people in the neighbourhood called *a bottom wind*. Dalton ſays, Darwent lake is agitated at certain times during a calm ſeaſon, by ſome unknown cauſe. Mr. Croſsthwaite has been aſſiduous in obſerving any circumſtance that might lead to a diſcovery of the cauſe; but his reſult is, that nothing has occurred yet to throw light on the ſubject. He then gives various dates when the lake was diſturbed; amongſt the reſt he ſays,

Aug. 9, 1789. The lake in very great agitation, white breakers upon large waves, &c. without wind.

Oct. 1792. The water much agitated.

My informant, a boatman of the name of Walker, ſays he has frequented the lake for about 27 years, and heard of perhaps 13 iſlands, though previous to that time for 20 years none had appeared. One roſe laſt year, and two this year, which came up within two days of each other. The firſt roſe the 11th of September, and part ſunk the 27th of the ſame month. Sometimes one comes up and is down again in 24 hours, and ſometimes they ſtay two months. One of them in the year 1798 was 180 yards long, 50 yards wide, and ſtaid at the top of the water ſix weeks. One was pierced, and found ſeven yards thick. They frequently burſt, and are rent ſo wide, that boats can ſail up and land paſſengers at the edges of them, to walk about on the iſland. One was a foot high perpendicular of land above the level of the water. They are connected always by one ſide to the graſſy turf at the edges of the lake. When, within a few days after their firſt appearance, a pole is run three to ſix feet into them, and drawn out again, the air for ſeveral ſeconds bubbles up like a pot boiling violently; a ſmell ariſes like gunpowder, and it has been ſaid that with a candle you might light a bottle-full that was once collected. I was this day rowed to one, a part of which continued above the water, which gradually deepened all round it, and found it of an oval ſhape, about 10 yards by five yards. The top was a fine firm mud, thick ſet with a young aquatic plant I did not know. We puſhed the boat-hook about four feet into it. The firſt part, about two feet, appeared all mud, then leſs firm, and full of partially decayed leaves and roots of trees and plants; the water bubbled up for a few ſeconds about two inches high, occaſioned by the air that followed the hook out.

The riſing of theſe iſlands is always accompanied with a bottom wind: yet the bottom wind appears frequently without them; and generally after a month or ſix weeks of dry weather the lake is ſometimes partially, ſometimes wholly agitated, accompanied by a roaring noiſe (the probable effects of echo in a calm in that ſituation). The waves are not long and rolling, but irregular ſhaped like mountains, 20 inches to two feet in height, like water jumbled in a tub; they frequently ſtrike the boat like a rock, and break in ſpray from head to ſtern, not always when it is perfectly calm, but alſo with a gentle breeze, barely enough to ripple the ſurface. They frequently indicate a change of weather from fair to rain, and the greateſt degree of agitation is always in the deepeſt water.

Having thus minutely deſcribed all the particulars I have time to collect, I ſhall leave it to others to find a better explanation than the following. When we conſider that half the bottom of the lake is covered with a fine mud impervious to air, reſting upon half-putrid vegetable matter; that being attached to the ſloping banks, all air that is generated will endeavour to riſe to the higheſt part, but cannot then get out; that great part of the bottom is a clean bed of pebbles and ſtones pervious to air, that the iſlands begin to riſe generally in from 9 to 12 feet water, and are of conſiderable ſurface; that they have been meaſured 21 feet thick, with clear water under them; and that the moſt violent effects have been in the ſummer months—Is it not probable that the air ſet at liberty by putrefaction is enough to raiſe theſe large maſſes, ſo thick, and ſo little heavier than water itſelf; to contract and expand their extremities enough to give the violent and irregular motion to the ſuperincumbent waters, by their flowing off, their reflux, and their breaking through—and that, more or leſs, whether they riſe to the ſurface, or only part of the way, to find their equilibrium in the ſurrounding fluid? Wiſhing our ignorance of the cauſe of this curioſity of our own country may ſoon be removed, and wiſhing ſucceſs to every inveſtigation of the phænomena of nature, I will conclude with the ſignature of

D. I. P.

XXIX. *Deſcription of a new Water Engine, invented by Dr.* West, *of the Iſland of Jamaica* *.

THE ſcrew-pipes *a* and *b* (ſee Plate V. fig. 1.) turn in different directions, and the area of the lower muſt be to that of the upper in the *inverſe ratio* of the perpendicular heights, and as much more as will be neceſſary to overcome the friction. The pipe *b* ought to widen a little from below upwards, in order to accommodate itſelf to the falling water.

Working of the Engine.

The valve of the pipe *a* at its upper orifice *c* being opened, (while the cup or baſon *f* is ſupplied by any ſtream of water, a part of which is wiſhed to be raiſed to a higher elevation,) the pipe *a* muſt be filled with water. It will continue full, though its lower orifice be open, if the valve fit cloſe; and, water continuing to be poured into the cup *f* from the pipe *d*, if a motion on the axis be then communicated to the pipes in a direction oppoſite to that of the ſteam iſſuing at *e*, the whole will continue to move round ſo long as water is ſupplied by the pipe *d*, and part of the water will aſcend in *a*, and be delivered into the upper circular gutter *g*, *g*.

XXX. *Deſcription of a new Hygrometer.*

To Mr. Tilloch.

SIR,

I HEREWITH encloſe a ſcheme for an hygrometer, which appears to me to be an improvement on the common (oat-beard) hygrometer in ſome reſpects, though, being more complex, it is more likely to be out of order. I have made ſeveral of them, and find they act tolerably well. Should you think the invention worth noticing in your publication, you are welcome to make what uſe of the paper you think proper. Two other kinds of hygrometers I have alſo made, but they do not anſwer ſo well as I think, with ſome improvement, they might be made to do. In one the index moves under the ſpirals of a watch ſpring, by which the number of revolutions may be reckoned; on account of the ſtiffneſs of the ſteel, the index does not move with freedom. In the other the index moves between the coils of a piece of paper cut in a ſpiral form, on which paper are figures marked to regiſter the place it points to. Both theſe inventions are for the ſame purpoſe as the winding hygrometer, which is to avoid the confuſion occaſioned by the oat-beard having more than *one* turn or twiſt in its courſe from wet to dry. I remain, Sir,

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* Communicated by Mr. Robertſon Buchannan, engineer, Glaſgow.

Your very humble Servant,
B. M. Forster.

Deſcription of the Winding Oat-beard Hygrometer.

The principal differences in this hygrometer from thoſe uſually made of oat-beards are the following:

The graduated circle (Plate V. fig. 2.) is numbered *completely round*, inſtead of *half round* each way, as uſual. On the top of the oat-beard * is cemented a circular piece of paper A, on which is fixed a tubular piece of ſtraw B, which is capped with another piece of paper C. D is a ſupport to keep the beard upright, made of card paper. On the ſtraw tube, or little cylinder, is faſtened a piece of fine ſilken ſtring, on which is hung a pea, to ſerve as a weight to keep the ſtring ſtretched. As the oat-beard untwiſts with moiſture, the index (made of ſtraw) moves the ſame way round as the hand of a watch, and, thus moving, coils the ſtring round the ſtraw tube or axis; by which means the number of revolutions from any time obſerved may be known; and thus the confuſion will be avoided which is occaſioned by the index moving more than once round, which it does in paſſing from extreme dryneſs to wet.

The ſtring may be ſo placed as to wind *up* when the index moves the contrary way, that is, from moiſt to dry, if the maker ſo chooſes; and in this caſe, the circle muſt be numbered the contrary way from the above.

The oat-beard is *fixed*, and the index is not to be turned or ſet to a certain point, as is the caſe with the common hygrometer, by a contrivance behind the caſe.

The method of keeping a regiſter of this hygrometer will be thus:

When on the upper part of the axis there is
One coil, and the index points at 6, ſet down 1—6
Two coils - - 6 - 2—6, &c.

If the circle be divided into 100 diviſions, the reckoning will be,
No coil, and the hand at 10, ſet down 10
1 coil - - 10 - 110
2 coils - - 10 - 210, &c.

which, if the circle be large enough, will be a very convenient mode of regiſtering.

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* The beard of the *avena ſterilis* (of Linnæus), which, on account of its ſize, is preferable to the beard of the common oat.

XXXI. *Hiſtorical Sketch of the Inſtitution and Progreſs of the Royal Society of London.*

[Continued from p. 38.]

EVEN within a few years after its inſtitution, the Society had acquired a conſiderable library, and a good collection of natural curioſities. A Mr. Colwal was, in theſe things, one of its firſt and moſt munificent benefactors. Mr. Hooke, the firſt curator of the Society, had the care of the books and curioſities; and they were arranged according to a claſſification of all the ſpecies in nature, contrived by the famous Dr. Wilkins. Mr. Henry Howard beſtowed on the Society the whole Arundelian library, conſiſting of ſome thouſands of printed books, with ſeveral hundreds of choice manuſcripts. From the time, alſo, of the great fire, after which the buildings of Greſham-college were again occupied as an exchange, Mr. Howard afforded the Society a place of temporary accommodation in Arundel houſe.

Among the communications to this Society, within the firſt ſeven years after its incorporation by charter, were many curious theoretical diſcourſes. Such were an hypotheſis of the motions of the moon, and of the ſea; a theory of fire and flame; an hypotheſis of the form and ſpring of the air; a diſcourſe of the poſſible height of the air, and its rarefaction upward; a diſcourſe about improving wood for dyeing, and for fixing colours; diſcourſes upon ſeveral mercurial experiments; a diſcourſe of annealing and tempering ſteel; diſcourſes about cyder and coffee; a diſcourſe of the poſſibility of the retardation of the motions of the heavenly bodies, and of their going ſlower and ſlower the longer they laſt;—with a multitude of others, which it were inconvenient and tedious here to enumerate in detail.

As their views were more expreſsly and directly than thoſe of their preſent ſucceſſors in the Royal Society to the improvement of the arts, they quickly formed accurate hiſtories of many of the moſt uſeful of theſe. Among their papers, within the firſt ſeven years after the Society's inſtitution, were to be found, an hiſtory of Engliſh mines and ores, two ſeparate hiſtories of tinneries and tin-working, hiſtories of iron-making, of lignum foſſile, of ſaffron, of alkermes, of verdigreaſe, of the bleaching of wax, of colours, of the making of alum, of the preparation of ſalt from ſalt water, of enamelling, of engraving, of varniſhing, of dyeing, of refining gold, of making potaſhes, &c. The proceſſes of making bread, of making cloth, of tanning with the other parts of the manufacture of leather, of paper-making, of hat-making, of brewing, were accurately related according to the beſt practice of them at that time, in different memoirs by the firſt members and correſpondents of the Society. Prince Rupert communicated two papers of extraordinary value, explaining the proceſs for the manufacture of gunpowder, and an improvement of that proceſs, by which gunpowder might be made which ſhould have twenty times the ſtrength of that which was commonly in uſe. Mr. Henſhaw produced to the Society an ingenious hiſtory of the diſcovery and the manufacture of ſalt-petre. Many papers illuſtrative of the hiſtory of the uſeful arts were communicated by Sir William Petty, one of the moſt truly honourable founders to whom any family among our Britiſh nobility can trace the origin of its wealth and nobilitating diſtinctions.

But none of all thoſe original members did more than Dr. (afterwards Sir) Chriſtopher Wren to advance the purpoſes of the Inſtitution. He propoſed certain curious theories of motions, extending and correcting the doctrine of Des Cartes, which, however they may have been ſince exploded, were imagined with great ingenuity, and illuſtrated by a multitude of happy experiments. He contrived to annex to a weather-cock an apparatus, which, of itſelf, regiſtered for every twelve hours the changes of the wind; and invented alſo a thermometer, to regiſter for itſelf the variations in the temperature to which it ſhould be expoſed. He was the inventor of an inſtrument for meaſuring the quantity of the rain that falls. He was the firſt who explained, that from the pendulum might be produced a natural ſtandard of meaſure for vulgar uſe. He firſt exactly meaſured and delineated the ſpheres of the humours in the eye; and thence explained how reflection conduces as much as refraction to viſion. He was the firſt author of the noble anatomical experiment of injecting liquors into the veins of animals. He made a variety of experiments to aſcertain the powers of the magnet; and many to explain the nature of the powers by which ſailing is performed, and to fix what fabric of a ſhip might be moſt ſuitable to the uſes for which it is intended. He made many improvements upon teleſcopes, and a prodigious number of aſtronomical obſervations, particularly on the planet Saturn, and on the moon.

He invented a curious and very ſpeedy method of etching. He ſuggeſted a number of contrivances for the improvement of water-works. He found out certain perpetual lamps and regiſters of furnaces, by which artificial heat might be ſo kept up and graduated as to imitate nature in the hatching of chickens and inſects, the vegetation of plants, the production of certain foſſils, and the preſervation of an equality in the motions of watches, in order to the diſcovery of the longitude, &c. All theſe things he accompliſhed in a diligent purſuit of the objects of the Society: and as his diſcoveries were made, he communicated them, but with the greateſt diffidence and modeſty, at its meetings.

And yet, even this variety, activity, and ſucceſs in its reſearches, could not ſatisfy the tyrannical impatience of public expectation. The cavillers againſt the Society derided its members as idle dreamers, or intereſted impoſtors, who excited hopes which they could not gratify, and waſted their own lives and fortunes in purſuits having no rational attainable end. Others conceived prejudices ſtill ſtronger againſt theſe ſtudies; accuſing them as atheiſtical, and adverſe to the ſtudy of purely moral and intellectual truth; becauſe they traced to ſecond cauſes phænomena, which had been uſually referred to the immediate agency of the Divine Power; and withdrew attention from the logic, ethics, and theology, which had hitherto reigned in the ſchools. By others they were abhorred as incapable of alliance with elegance and grace, with the charms of poetry and eloquence, with the refinement of the language, or the improvement of the popular parts of literary compoſition. To many that tolerance was extremely odious with which they received all men of ingenious inquiry into their correſpondence, without diſtinction of nation or religion; and the admirers of the ages of claſſical antiquity derided, with the proudeſt contempt, the very idea upon which the Inſtitution was founded,—that it was poſſible to extend the empire of human knowledge beyond thoſe limits within which the antients had left it confined.

The Society, not unmoved by ſuch cenſures and complaints, yet not diverted by them from the proſecution of its views, advanced, though ſlowly, yet with ſteady and vigorous diligence, in its experiments and inquiries. Dr. Spratt's apologetical hiſtory contributed to ſilence many prejudices. The imitation of the deſign of this Society, by ſo many ſimilar inſtitutions ariſing after it in foreign countries, evinced the general conviction of mankind to be in its favour. Its memoirs and tranſactions were, from time to time, printed; and, among diſcoveries and inquiries which could intereſt none but philoſophers, failed not to contain, likewiſe, others of general utility and importance ſo ſtriking and palpable, that even the attention and approbation of the vulgar could not be refuſed.

In pure mathematics, the ſucceſſive publications of the

Society, to the end of the ſeventeenth century, exhibited a variety of inventions exalting the power of that ſcience, and furniſhing it with new arms to triumph over difficulties of inveſtigation to which it was before unequal. The diſcoveries of Newton had been prompted by that ſpirit of mathematical and phyſical reſearch which the inſtitution of the Society excited. Their communication did honour to the meetings of the philoſophers, and enriched their volumes. Wallis, Gregory, De Moivre, and Halley ſoon communicated a number of valuable problems, theorems, and new demonſtrations, important both in the mere diſplay of beautiful, and in their ſuſceptibility of application to the improvement of the mathematical arts. In mixed mathematics the optical diſcoveries of Newton, Hevelius, and Huygens were, in great part, made firſt known to the world through the medium of the Royal Society. Leeuwenhoek and the illuſtrious Dr. Robert Hooke publiſhed moſt of their microſcopical diſcoveries, in the firſt inſtance, through the ſame channel. A multitude of the moſt important obſervations in aſtronomy, the communications chiefly of Flamſteed, Hevelius, Caſſini, Halley, Gregory, Bullialdus, Auzout, fill the early volumes of theſe Tranſactions. Bernoulli, Huygens, Hooke, and Wallis communicated ſome highly curious papers in mechanics and acouſtics. Mr. Boyle, M. Homberg, Dr. Papin, Dr. Reiſelius, and an inferior philoſophical ſociety at Oxford, communicated various experiments on the gravities of different fluids, their ſuperficial figures, and their laws of movement. Mr. Thomas Savery, in the year 1699, made known to them an engine of his invention for raiſing water by ſteam.

Flamſteed, Borelli, Halley, Mercator, Caſſini, Bullialdus, and Greaves, gave, for the improvement of geography and navigation, many obſervations of the longitudes and latitudes of different places, of the variations of the compaſs, &c. &c.

In architecture and ſhip-building, their Tranſactions exhibit the valuable papers of Leeuwenhoek "on the differences in timber as it grows in different countries, and is felled in different ſeaſons;" of Mr. Bulteel "on the ſheathing of ſhips with lead;" of Dr. Liſter, Dr. T. Robinſon, and Dr. Wallis, "on bridges, arches, and chimneys, &c." Wallis and Salvetti communicated ſome intereſting papers on the theory of muſic. Dr. Sherard gave a receipt for making china varniſhes.

In phyſiology, meteorology and pneumatics, the reſearches of this Society in the laſt thirty years of the ſeventeeth century were no leſs diligent and meritorious The barometer, the hygrometer, the thermometer, were firſt put to important uſe in philoſophical obſervation by its members.

The general phænomena of the weather in different ſeaſons and latitudes were by them firſt recorded with philoſophical accuracy. Of the winds, rains, ſnow, hail, lightning, thunder, meteors, exhalations, their papers exhibit an aſſemblage of facts, to which much has indeed been ſince added, but which ſtill form a large and important part of our preſent ſcience of atmoſpherical phænomena. They collected a prodigious variety of obſervations to illuſtrate the phyſical hiſtory of water, as it exiſted in ſeas, lakes, ſprings, mineral and ſalt-ſprings, &c.; and to explain the manufacture of ſalt from ſea water or from that of ſalt-ſprings.

In thoſe early volumes are a very great number of papers in mineralogy: the facts which they contain laid the foundation of this ſcience. The hiſtory, in particular, of animal ſubſtances, and of vegetables found in a foſſil ſtate; of ſome volcanic eruptions; of marble found in Ireland; of the formation of peat-earth in Scotland; of the ſtrata of pit-coal; of rock cryſtal, iron, and copper-ore; of amber; of the electrical capacities of amber, gum lac, and diamonds; was illuſtrated in a number of curious papers, the produce of much laborious inquiry, and of many obſervations made with the greateſt diligence. Mr. Gill, Sir Robert Moray, Dr. Brown, and others, communicated intereſting papers on damps in mines. Dr. Liſter gave ſome mineral maps, and an account of the true way of making ſteel. A catalogue of electrical bodies was received from Dr. Robert Plot: Mr. D. Colwal favoured the Society with accounts of alum works and green copperas works. The lead mines in Somerſetſhire, the quickſilver mines in Friuli, the ſilver and gold mines in Hungary, were made particularly known to the Society by communications from Mr. Glanville, Dr. W. Pope, and Dr. E. Brown. A curious paper from Mr. M. Septalius acquainted them with the exiſtence of mercury in certain plants. The collection of their papers preſents, beſide theſe, a prodigious variety of other information relative to ſubjects in the mineral kingdom.

The power of the magnet; the uſe of the magnetic needle; the declination of the needle, its variations in different places, with the theory of thoſe variations, engaged much of the Society's attention; and were very ably illuſtrated by the papers of Dr. Halley, Mr. Hevelius, Mr. Auzout, Mr. Cunningham, and others.

To the improvement of the ſcience of botany, and the practice of agriculture, they continued to devote much of their care. Rye, turnips, potatoes, maize, ſaffron, orange-trees, vines, are among the plants whoſe culture and œconomical uſes were ſucceſsfully explained in the papers read before this Society during the laſt century. The nature of vegetation, the circulation and deſcent of the ſap in trees, the reunion of bark to trees from which it had been ſeparated, &c. were finely explained by Dr. Beale, Dr. Liſter, Mr. Reed, M. Malphigi, Mr. Ray, &c. The uſe of marle, of ſea ſand, of ſalt and of brine for manures, are taught in others of thoſe papers. Mr. King communicated a method of improving the bogs and loughs in Ireland by drainage. Dr. Beale explained the fertilizing powers of froſt. The preparation of oak for tanning, of ſugar from the maple-tree, of vinegar, of pitch, tar, reſin, and turpentine, are the ſubjects of ſome other ingenious papers of the ſame period. Dr. Liſter inveſtigated with curious pains the botanical hiſtory of the muſhroom. Hemlock, opium, ſnake-root, aloe americana, the amomum of the Philippine iſles, and many other plants adapted to be of uſe in the materia medica, were examined in their growth and qualities, under the Society's directions. Dr. Liſter proved the poſſibility of fertilizing a ſandy ſoil by the addition of clay. Sir Robert Moray gave a valuable paper on the proceſs of malting. Dr. Beale and Mr. Dale taught how to make bread from turnips and potatoes.

In zoology, the papers of this period are likewiſe numerous, and illuſtrative of many of the moſt intereſting ſpecies in the animal kingdom. The hiſtories of the ſilkworm, the whale, the cochineal inſect, the pearl-muſcle, the bee, were, on account of the relations of theſe animals to the uſeful arts, examined with extraordinary care. Many accounts of monſtrous births and figures of animals were from different parts tranſmitted for the Society's conſideration. Waller's poem "on the Summer Iſlands" appears to have had for its foundation a narrative concerning whales and whale-fiſhing at the Bermudas, which was communicated to the public in the Tranſactions of the Royal Society. Mr. Ray, examining into the nature of ants, diſcovered and made known to the Society that which has been ſince named the *formic acid*. Others, with much curious pains, illuſtrated the hiſtory of the ſpider. Dr. Liſter found an acid juice in another inſect beſide the ant. Mr. Boyle and others examined and explained the anatomical ſtructure of fiſhes. In regard to the human body eſpecially were theſe philoſophers wonderfully induſtrious in reſearch. The natural accidents to which it is ſubject, and its anatomical ſtructure, were never before ſo well illuſtrated as by the facts which they collected. Concerning the ſtructure, external parts, and common teguments of human bodies; the head; the neck and thorax; the abdomen; the humours and general affections of the body; its bones, joints; their Tranſactions preſent an aſſemblage of facts ſuch as muſt, in compariſon, put to ſhame the induſtry of later anatomiſts.

In chemiſtry, they inveſtigated the nature and compoſition of phoſphorus, of the Bologna ſtone, of vegetable and mineral acids, &c. An engine to conſume ſmoke was made known to them by M. Juſtel. A method of imitating the pottery of China appears alſo among their papers. Sir Robert Southwell communicated an account of the method of dreſſing buck and doe ſkins, which was practiſed by the Caribbees in the Weſt Indies. The phoſphoreſcent qualities of wood, putrid fleſh, the ſurface of the ſea, &c. were particularly examined in papers communicated by Mr. Boyle and Dr. Beale.

Nor were their endeavours confined to the improvement merely of phyſical and mathematical ſcience. Mr. Lodwick gave an eſſay towards an univerſal alphabet. Dr. Wallis invented a method of teaching perſons deaf and dumb to ſpeak and underſtand language. Mr. Edward Lloyd communicated ſome valuable obſervations in philological ſcience. Another gentleman gave a paper of highly curious obſervations and conjectures concerning the Chineſe characters. The collection of their papers affords likewiſe ſome valuable illuſtrations of difficulties in chronology. Roman, Grecian, Saxon, Runic, Egyptian, and Perſian antiquities have alſo a variety of new lights thrown upon them in theſe papers. The firſt account of the diſcovery of the famous ruins of Palmyra appears here in two communications from Mr. Timothy Landy and Mr. Aaron Goodyear. When the members and correſpondents of the Society were engaged in journeys and voyages, they never failed to regiſter for its information at leaſt ſome of the more extraordinary facts which came under their notice. And a number of its papers are, in conſequence of this, narratives of ſuch obſervations, intereſting in the higheſt degree at once to philoſophers and to mere popular curioſity.

Yet about the beginning of the 18th century the members of the Society were themſelves diſſatisfied with the progreſs and ſucceſs of their efforts. They began to feel that their firſt ardour of inquiry had become cool. They complained, that "the diſcouraging neglect of the great, the impetuous contradiction of the ignorant, and the reproaches of the unreaſonable, had unhappily thwarted them in their deſign to perpetuate a ſucceſſion of uſeful inventions." Nor was it to have been expected that the fears for religion and liberty in the latter part of the reign of Charles the Second and his ſucceſſor, the civil and political contentions to which they gave birth, and the direction of ſo much of the learned ability of the time, to exerciſe in the field of the popiſh controverſy, ſhould not have proved inauſpicious to the advancement of philoſophical ſtudies. The reign of William, haraſſed with wars, with diſputes relative to the rights of the reigning ſovereign and to the royal ſucceſſion, with factions in church and ſtate, introduced no new fortunate æra for the advancement of philoſophy and the arts. Even in the beginning of the 18th century, and during the courſe of ſome years, the ſame diſſenſions and wars continued to produce the ſame effects upon the fortune of ſcience. The wits preſumed to throw ridicule upon ſcience and erudition, which they were too idle to cultivate. The reign of Anne was for England perhaps the golden age of wit and elegant literature; and men ſhowed themſelves ſufficiently diſpoſed to prefer the light and ſhining, to that which was only ſolid and unoſtentatious.

Yet, while Newton, the pride of the Royal Society, diſtinguiſhed himſelf among its active members, or preſided at its meetings, it could not but continue to be regarded as the grand focus of phyſico-mathematical diſcovery and knowledge. His own communications in optics, aſtronomy, and general phyſics, were invaluable. His ſyſtem of the univerſe was adopted as one which was infallibly true, and which it was for the honour of the nation, by all poſſible means, to illuſtrate and maintain. Hence, from the commencement till nearly the middle of the 18th century, the labours of the Society were principally employed in purſuing the analogies of the Newtonian ſyſtem throughout all that diverſity of phænomena by which its certainty was to be tried. Such were almoſt all the experiments, obſervations, and theories, of the illuſtrious, the indefatigable Halley. Deſagulier, Facio, Keill, and Maclaurin, ſucceſsfully laboured in the ſame field. Every other branch of thoſe ſciences which the Society ſtudied eſpecially to improve, continued alſo to be more or leſs advanced by the labours of its members.

It was at length evident that the field which its founders had, with noble ambition, marked out for themſelves was too vaſt. The Society of Arts aroſe by perhaps an unconſcious derivation from the Royal Society. To the latter was ſtill left the province of the ſublimer and abſtruſer ſciences. The latter, with admirable national enthuſiaſm, undertook the taſk of applying, and encouraging the application of, ſcientific truth to the improvement of the common arts.

For a time, about the middle of the 18th century, when the Newtonian ſyſtem was fully eſtabliſhed, and no new path of noble diſcovery had been opened, the exertions of theſe philoſophers might comparatively languiſh. The diſcoveries of Dr. Stephen Hales concerning the diverſities of airs, about that time, however, renewed the truths which had been firſt explained by Hooke. Diſcoveries in natural hiſtory and chemiſtry were continually more and more multiplied. Franklin at length communicated to the Society the grand truth of the identity of lightning with electricity, and a new theory combining all the electrical phænomena which had been as yet obſerved. Prieſtley, following Hales, Hooke, and Boyle, in experiments upon air, diſcovered all the varieties of aëriform ſubſtance. Cavendiſh, Kirwan, and others, examined airs in their relations to the calces of metals, &c. Sir Joſeph Banks has done high honour to the Society by the advancement which it has, under his auſpices, made in botanical diſcovery, and in the culture of the other branches of natural hiſtory. At the preſent time, its labours are, in all the branches of phyſical and mathematical ſcience, moſt zealouſly continued. Amid ſo many rival inſtitutions the Royal Society of London holds ſtill the firſt place. Its memoirs, now with great regularity annually publiſhed, fully vindicate its claim to the higheſt eſtimation of the public. We have reviewed its hiſtory with a conſcious pride that we are of the ſame country and language with a ſucceſſion of philoſophers who have diſcovered, or collected and arranged, the better part of the phyſical knowledge peculiar to modern times. He who would aſpire to the praiſe of a philoſopher would do well to ſtudy the volumes of its Tranſactions with peculiar care.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

THIS learned body had its firſt meeting after the ſummer receſs on the 5th inſt. (November), which was occupied by the reading of the Croonian lecture by Everard Home, Eſq.

The meetings on the 12th and 19th were taken up in reading the Bakerian lecture on the theory of light and colours, by Dr. Young, profeſſor of natural philoſophy at the Royal Inſtitution.

In this lecture the author offers no new experiments, a ſufficient number for his purpoſe having been made and recorded by preceding philoſophers, on which he reſts four hypotheſes: The firſt is, that a *luminiferous æther*, of an extreme degree of rarity, pervades the univerſe; the ſecond, that, when bodies become luminous, *undulations* are excited in this æther—The third, that colours are owing to *vibrations* in this æther acting on the retina—and the fourth, that all matter attracts this æther; and that, when bodies are luminous, it acts in a more denſe, though not in a more elaſtic ſtate upon them.

In the meeting on the 26th was read an analyſis of a mineral from North America, which contains a metal different from any hitherto known: by Charles Hatchet, Eſq.

This is an acidifiable metal, but differs in ſome reſpects from thoſe acidifiable metals already known. A vivid orange-coloured precipitate can be thrown down from one of its ſolutions, which, as it remains unchanged after being expoſed to air and light, will be a moſt valuable acquiſition to artiſts. By another proceſs a green precipitate is obtained.

It is propoſed to give this metal the name of *Columbium*. The ſpecimen from part of which this analyſis was made is in the Britiſh Muſæum; but Mr. Hatchet is in hopes of being able to obtain a freſh ſupply from America.

Part II. of the Philoſophical Tranſactions for 1801, juſt publiſhed, contains the following articles:

12. A Hiſtorical and Anatomical Deſcription of a doubtful amphibious Animal of Germany, called by Laurenti *Proteus Anguinus*. By Charles Schreibers, M. D. of Vienna.—13. Obſervations tending to inveſtigate the Nature of the Sun, in order to find the Cauſes or Symptoms of its variable Emiſſion of Light and Heat; with Remarks on the Uſe that may poſſibly be drawn from Solar Obſervatons. By William Herſchel, LL. D. F. R. S.—14. Obſervations on the Structure, and Mode of Growth, of the grinding Teeth of the Wild Boar, and Animal incognitum. By Everard Home, Eſq. F. R. S.—15. Accounts of ſome Experiments on the Aſcent of the Sap in Trees. In a Letter from Thomas Andrew Knight, Eſq. to the Right Hon. Sir Joſeph Banks, Bart. K. B. P. R. S.—16. Additional Obſervations tending to inveſtigate the Symptoms of the variable Emiſſion of the Light and Heat of the Sun; with Trials to ſet aſide darkening Glaſſes, by tranſmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made againſt ſome of the Arguments contained in the former Paper. By William Herſchel, LL. D. F. R. S.—17. On an improved Reflecting Circle. By Joſeph de Mendoza Rios, Eſq. F. R. S.—18. Obſervations and Experiments upon *Dr. James's* Powder; with a Method of preparing, in the humid Way, a ſimilar Subſtance. By Richard Chenevix, Eſq. F. R. S. M. R. I. A.—19. Caſe of a young Gentleman, who recovered his Sight

when ſeven Years of Age, after having been deprived of it by Cataracts before he was a Year old; with Remarks. By Mr. James Ware, Surgeon.—20. An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. Humphry Davy, Lecturer on Chemiſtry in the Royal Inſtitution.—21. A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on Solar Light, when imbibed by Canton's Phoſphorus. By Nathaniel Hulme, M. D. F. R. S. and A. S.—22. Experiments on the chemical Production and Agency of Electricity. By William Hyde Wollaſton, M. D. F. R. S.—23. Farther Obſervations on the Effects which take Place from the Deſtruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafneſs. By Mr. Aſtley Cooper.

THE SOCIETY OF ARTS AND SCIENCES AT UTRECHT.

This ſociety has propoſed the following

Prize Queſtions:

1ſt, What is the real nature of the electrical matter? Is it compound, and of what principles is it compoſed? What are the chemical changes which it experiences by its union with other bodies, and which it produces in theſe bodies?

The beſt anſwer, to be tranſmitted before the 1ſt of October 1803, will be entitled to a medal of the value of thirty ducats.

2d, The following, which has not yet been anſwered, is again propoſed:—What is the real nature of the dyſentery? Is it always the ſame? What are the different cauſes of this diſeaſe? What are its ſymptoms, and their conſequences? What is the beſt treatment, and method of cure? and, in particular, how far is the uſe of opium proper as a remedy? The anſwer to be ſent in before the 1ſt of October 1803.

3d, A medal, of twenty ducats value, will be adjudged to the author of the beſt paper on natural hiſtory, and a ſilver medal to the next beſt.

The papers to be ſent in before the 1ſt of October 1802.

4th, Why are the diſeaſes now prevalent in Holland not ſo ſimple as formerly? Is it owing to bilious, viſcous, or other cauſes? By what means can it be aſcertained with moſt certainty, in the commencement of theſe diſeaſes, which of the above cauſes predominate? What is the beſt method of cure?

The papers to be tranſmitted before the ſame period.

5th, By what means can duelling be prevented in a country where an idea prevails, that infamy attaches to a perſon who on receiving certain injuries does not call forth the offender, or who when called forth does not accept the challenge? And in ſuch caſes, what meaſures are to be purſued?

The papers to be ſent in as before.

The anſwers to theſe queſtions, written either in the German, Dutch, Engliſh, French, or Latin languages, muſt be inſcribed with a certain device, and tranſmitted, free of poſtage, to Dr. Luchtmans, at Utrecht, ſecretary to the ſociety.

FRENCH NATIONAL INSTITUTE.

Notice of the labours of the Claſs of the Mathematical and Phyſical Sciences during the laſt three months of the year 9.

Mathematics.

C. Delambre read the following account of the mathematical labours:

A memoir on the equilibrium of arches, by C. Boſſut, was read. As the ſtones which compoſe an arch mutually counterbalance each other, they remain ſuſpended without any ſupport from below, all their effort being directed towards the abutments, which ſupport the arch, as if it formed one continued body.

To enſure the duration of an arch, it is not ſufficient that all the ſtones ſhould be in equilibrium; the abutments on which they reſt muſt oppoſe a ſufficient reſiſtance to the efforts the arch makes to overturn or cruſh them.

A reſearch into the means proper for preventing their being overturned conſtitutes what is called the problem of the puſh of arches, which engaged the attention of ſeveral geometricians of the laſt century; but they entirely neglected that problem which has for its object the means of preventing cruſhing.

To try the reſiſtance of which different ſtones are ſuſceptible, Soufflet invented a machine, which was afterwards improved by C. Rondelet.

The antients had no very certain or geometrical principles for inſuring the ſolidity of their edifices; at leaſt, no trace of them is ſeen in the work of Vitruvius, nor is any thing found in that work on the figure of the ſtones or on that of the wood; which induces C. Boſſut to think that their architects, attending merely to decoration and external form, left to the builders what related to the conſtruction and ſolidity, in which, unfortunately, they have had too many imitators among the moderns.

La Hire was the firſt, at leaſt in France, who determined, by the theory of the wedge, the proportion according to which the weight of the arch ſtones ought to be augmented in a ſemi-circular arch.

In 1704, Parent determined by points the figure of the extrados of an arch the intrados of which is a ſemi-circle. He determined alſo the puſh of ſuch an arch on the abutments.

James and John Bernoulli, Huygens, and Leibnitz, having reſolved the problem of the catenarian curve, it was ſoon perceived that this curve was that of the equilibrium for an arch compoſed of arched ſtones infinitely ſmall and equally heavy. David Gregory ſhowed this fact, and it is deduced ſtill more directly from a method given by James Bernoulli. A ſecond method of the ſame author, corrected by C. Boſſut, conducts to the ſame concluſion.

La Hire in 1712 gave a method, founded on ſome experiments, which, on account of its great facility, was adopted by practical workmen, who gave themſelves no trouble to inquire whether it was applicable to all caſes. Not content with following it in ſemi-circular arches, they applied it even to domes, though the equation of the equilibrium in this caſe is of the third degree, and not of the ſecond, as in the preceding caſe.

Couplet has treated of the puſh of ſemi-circular arches, and of the thickneſs of their abutments; firſt, by conſidering the arch ſtones as infinitely ſmooth, and experiencing no reſiſtance from friction; and then he endeavours to correct what may be defective in this ſuppoſition: but he does not go further than Parent and La Hire.

Bouguer has treated of domes. He has ſhown that a variety of curves may be employed in them, and of theſe he points out thoſe moſt advantageous. But he has not calculated the puſh, nor has he examined the law of the forces which muſt act on the arch ſtones when the generating curve is ſubjected to given conditions; a ſubject fertile in curious and uſeful problems.

In 1770 C. Boſſut undertook to treat of the queſtion in its whole extent, both in regard to ſemi-circular arches and domes. He examined every thing that relates to the form and puſh of arches. His memoirs were printed in the Tranſactions of the Academy of Sciences for the years 1774 and 1776.

Maſcheroni, who was deputed by the Ciſalpine republic to aſſiſt in fixing the unity of the new meaſures, and of whom the ſciences were deprived laſt year, made in 1785 new reſearches, and gave ſome ingenious propoſitions reſpecting the equilibrium of domes with circular elliptical and polygonal baſes.

After many new reflections and ſeveral experiments, which may be of the greateſt uſe in practice, C. Boſſut has reſumed his two memoirs, which he has thrown into one, ſimplifying his calculations in ſeveral places. He has made a great many additions both in theory and practice, ſo that the whole now forms a work which may be conſidered as new.

Comet.

C. Meiſſier read a note on the comet which he diſcovered on the 12th of July laſt, about half after 11 in the evening. It had a very faint light: in 41 minutes time it had 24′ 40″ direct motion in right aſcenſion, and 6′ 38″ in northern declination decreaſing. The ſame comet was ſeen the ſame day, and almoſt at the ſame inſtant, by Mechain and Bouvard; and the latter obſerved it on the meridian at 11 h. 57′ 49″ true time. Its right aſcenſion was 111° 15′, and its northern declination 69° 30′.

It has ſince been known that it was obſerved the ſame day at Marſeilles by C. Pons, who had even ſeen it the day before. But the clouds did not then permit him to aſcertain, by regular obſervations, whether it was a comet or only a nebulus. Mechain has calculated its elements.

Obſervation of the Summer Solſtice of the Year 9 (1801).

Duc-la-Chapelle, aſſociate, has communicated to us the reſult of the obſervations he made at Montauban, to determine the ſolſtitial altitude of the ſun and the obliquity of the ecliptic.

By the mean of nine days obſervations he found the appa-ent obliquity to be 23 28′ 9″, ſuppoſing the ſemi-diameter of the ſun 15′ 48″, and the latitude of his obſervatory 44° ′ 52″.

Theſe obſervations were made with Lacaille's ſextant; and it would perhaps be proper to take from the tables of that ſtronomer the diameter of the ſun and the refractions which e determined with the ſame inſtrument, becauſe theſe tables ontain alſo the errors of that ſextant. By theſe means the bſervations of Duc-la-Chapelle would agree with ours. By mean of eighteen days obſervations, made with Borda's rcle, I found the obliquity to be 23° 28′ 8″2. Mechain and Le François found the ſeconds to be only from 6 to 7. It is exceedingly difficult to avoid theſe ſmall differences. Uniting my obſervations for three years, I find, by taking a mean, that the apparent obliquity at the ſummer ſolſtice of the year 8 (1800) was 23° 28′ 0″4, or 23° 27′ 58″ for the mean obliquity. The obſervations of Bradley, Mayer, Lacaille, and Le Gentil, gave nearly 23° 28′ 18″ for 1750; it would thence reſult that the ſecular diminution is only 40″ inſtead of the 50″ which are generally ſuppoſed. A part of the difference may ariſe from the obſervations, and particularly the old obſervations, which perhaps ought to be examined with the more correct elements which are employed at preſent.

Remarks on the Fifty Thouſand Stars, the Obſervations of which have been publiſhed by Lalande.

Lalande has announced, in the preface to his *Hiſtoire Celeſte*, that there are in the heavens a great many vacant ſpaces, many changing ſtars, and many red ſtars. He has now given further details reſpecting theſe objects in a memoir accompanied with tables. By vacant ſpaces Lalande here underſtands ſpaces in which no ſtars are ſeen of the ninth magnitude, which are the ſmalleſt that can be perceived with an achromatic teleſcope of ſixty-ſeven millimetres aperture (about 2½ inches) for illuminating the object-glaſs that the threads may be ſeen. It is beyond all doubt, that, by excluding all foreign light, and employing the ſtrongeſt magnifying glaſſes, theſe vacant ſpaces, properly ſo called, would be found to be conſiderably diminiſhed in number; and perhaps there is not in the whole heavens a ſingle place to which a teleſcope could be pointed without ſeeing a great number of ſtars, but below the ninth magnitude, and conſequently too faint to be of any uſe in aſtronomy.

Lalande gives a catalogue of all theſe vacant ſpaces; that is to ſay, his table contains the right aſcenſion and declination of the centre of each of theſe ſpaces.

The changing ſtars are contained in a ſecond table, and are in number thirty-one. There are only twelve the periods of which are known, but there are ſeveral others which decreaſe in magnitude ſo as to diſappear at intervals. By attentive obſervation, the time which elapſes between two conſecutive diſappearances might be determined; and this kind of obſervation Lalande recommends to the curioſity of thoſe who, being poſſeſſed of only moderate inſtruments, may, however, be deſirous of being uſeful to aſtronomy.

The third table contains thirty-three ſtars of a red colour.

About the year 1756 Mayer had remarked this tint in the nineteenth of *piſces*, which he denotes in the regiſter of his obſervations by the epithet of *rubicunda*, as we have ſeen by the copy ſent to us by M. Lichtemberg of all the obſervations made by Mayer on the day on which he obſerved the planet of Herſchel. Michell and Bailli ſuppoſe that the colour of the ſtars may depend on the different intenſity of their fire, or the degree of their inflammation; and that the red colour may indicate a fire decreaſing. Under this idea it might be of importance to examine the changes of colour which the ſtars experience. However, theſe variations, if they exiſt, are doubtleſs exceedingly ſlow; for the different ſhades at preſent remarked between Antares, Arcturus, Aldebaran, Syrius, and the Lyre, exiſted in the time of Ptolemy.

Extract from a Memoir on the Degree of Magnetiſm acquired by Bars of Steel of different Thickneſs, and on ſome Reſults in regard to the Magnetic Needle, by C. Coulomb.

Almoſt all the magnetic phænomena can be ſubjected to calculation if we ſuppoſe in ſteel two magnetic fluids, in each of which the moleculæ repel each other in the inverſe ratio of the ſquares of the diſtances, and attract in the ſame ratio the particles of the other fluid. This law was proved by Coulomb in the Memoirs of the Academy for 1786, from experiments which appear deciſive.

When ſteel is in its natural ſtate, and not magnetized, the two fluids are neutralized by each other; that is to ſay, they keep each other mutually in equilibrium, and no longer exerciſe any action, ſince one of the fluids attracts a magnetic molecula with the ſame force as the other repels it.

When the fluids are ſeparated they tend to unite and to neutralize each other; and they would indeed unite were there not a coercive force which oppoſes this union. This force may be either the adheſion of the moleculæ of the fluid to the ſteel, or the friction which they experience in paſſing from one point to another.

The author has proved (*Mémoires de l'Academie* 1787, p. 491) that whether the two fluids were ſeparated and carried each to an extremity of the bar, or whether they were only diſplaced in each molecula of the ſteel without the power of paſſing from one molecula to another, calculation gave the ſame reſult. He has proved at the ſame time that the latter ſuppoſition is the only one which can agree with experience.

According to theſe ſuppoſitions, a bar of ſteel may acquire every degree of magnetiſm, provided that there is no point in it where the reſult of the action of all the moleculæ of the fluid of the bar which act on the magnetic fluid of that point, is greater than the cohеſion which prevents it from being diſplaced.

But among all the degrees of magnetiſm there is one which may be called the point of ſaturation, or the *maximum* of magnetiſm; it is that where the reſult of the forces on each molecula is preciſely equal to the adheſion of that molecula to the ſteel. It may be readily conceived that when it attains to this ſtate the bar has the higheſt degree of magnetiſm which it can acquire; it is alſo that degree which ought to be given to bars deſtined for making needles to the mariner's compaſs.

To know whether a needle be magnetized to ſaturation, practice ſeems to furniſh but one method, which is, to magnetize it with bars of different ſtrength. If a bar, for example, be ſucceſſively magnetized with bars the force of which is as 1 to 2, and if the bar the firſt time has the ſame directive force as when magnetized with the ſtrongeſt magnets, it appears that we may thence conclude, with a great deal of probability, that the bar is magnetized to ſaturation. Theory furniſhes us with a more direct method, which is, to find by calculation what may be the directing force of a bar magnetized to ſaturation, and to examine whether the reſult of this calculation be agreeable to experience. It was in this manner that Coulomb proved, in the third volume of the Memoirs of the Inſtitute, p. 176, that if two bars of ſteel, all the ſides of which are homologous, that is to ſay, the correſponding ſides of which are in the ſame ratio, be compared with each other, theſe bars, ſuſpended horizontally as the needles of a compaſs, will have the momenta of their directing forces as the cube of one of their dimenſions. This reſult perfectly agrees with what has been found by experiment; which affords a new confirmation of the theory.

In the preſent memoir the author endeavours to determine the magnetic ſtate of ſeveral bars united in ſucceſſion one above the other, or, what amounts to the ſame thing, to determine the force of different bars in regard to their thickneſs.

But it may be eaſily conceived that, if the theory adopted is agreeable to nature, when two bars of great breadth, and very thin, are magnetized to ſaturation, each point magnetized, in the two bars united, will experience, on the part of theſe two bars, an action almoſt double to that which the ſame point would experience if each bar magnetized to ſaturation were inſulated. Thus, by uniting the two bars, each of them ought to retain only a little more than half the magnetiſm which it had when inſulated, ſince the adheſion is the ſame for two bars as for one. This, indeed, the author proved by taking two bars of a white temper of 80 millimetres (3,14960 inches) in length, a third of a millimetre (0·01312 inch) in thickneſs, and forty millimetres (1,57480 inch) in breadth. Experience ſhowed that the ratio of the momentum of the directing force of one inſulated bar to that of the ſame bar united, is as 185 to 100, or nearly, as may be ſeen, the ratio of 2 to 1.

But it may be readily conceived that, in proportion as bars have more thickneſs and leſs breadth, as the fluid condenſes itſelf in the edges and angles to which it is impelled in one direction without experiencing reaction, it muſt then happen that this ratio will be diminiſhed. In this reſpect, experience gave a reſult of great importance to practice. It is a ratio almoſt conſtant; ſo that, if a bar a millimetre (0·03937 inch) in thickneſs be joined to another of the ſame thickneſs; if theſe two bars be magnetized to ſaturation ſeparately, and if they be then magnetized together united, the ratio of the momentum of their directing forces in theſe two caſes will be very nearly as 165 to 100.

This ratio takes place alſo, as theory indicates, and as is confirmed by experience, whether the bars be hardened of a white temper, or whether they be tempered to the ſame degree of ſpring, provided they are compared in the ſame ſtate of temper and hardneſs.

It muſt however be remarked, that either by the method by which we magnetize, or ſome other property of the magnetic fluid which theory has not yet appreciated, when a bar very long compared with its other dimenſions is magnetized, the whole fluid proceeds towards its extremities in an extent ſo much narrower as the breadth and thickneſs of the bar are leſs, and as the degree of temper is greater. Between theſe two parts there is an interval which gives ſcarcely any ſign of magnetiſm; whence it happens, that by joining two very long bars the preceding law cannot have place, becauſe the action of the magnetiſm of the two bars united only cauſes the magnetic fluid to flow back into the parts which gave no ſign of magnetiſm when each needle was inſulated.

It is only with needles then which give ſigns of magnetiſm throughout their whole length that we ought to find an agreement between experience and theory. It is always found, for example, that if the directive force of an inſulated bar of two hundred millimetres (7,87400 inches) in length, whatever may be its other dimenſions, is to the directive force of the ſame bar, when united with one equal to it, as 165 to 100; all ſhorter bars will give the ſame reſult, and all bundles formed of ſuch bars will give, in regard to a bundle double in thickneſs, the ſame reſult.

Theory indicates alſo, that when a bar is compared with itſelf in different ſtates, whatever may be its dimenſions, if it be magnetized to ſaturation, its directive force ought to be augmented with the coercive force which keeps the magnetic fluids ſeparated. But as this coercive force is conſtant for equal ſtates of the ſteel, it thence reſults, that whatever may be the dimenſions of a bar it will give a conſtant ratio when its directive force is compared in two different ſtates. Here, alſo, experience is agreeable to theory. Bars made from the ſame piece of Engliſh ſteel-plate, whatever their number and dimenſions, when hardened at a cherry red, gave directive forces greater than when brought down from a higher to that temper in the ratio of 24 to 10, which announces that the coercive forces are in the ſame ratio.

The ſame bars brought to a blue temper gave, in regard to thoſe brought down to a white temper, the ratio of 164 to 100; but in this there is a little variety, becauſe it is almoſt impoſſible to enſure the ſame degree of tempering.

In ſteel of inferior quality there is ſtill more advantage in giving it a greater degree of temper. The author found bars of German ſteel the coercive force of which, when they were tempered at a cherry red, was to the ſame force, when brought from a higher to the ſame temper, as 34 to 10; but, on the other hand, this ſteel brought to a white temper has a directive force much leſs than ſteel of the Engliſh plates.

C. Coulomb has read to the claſs only this firſt part of his memoir.

The ſecond part is deſtined to determine the curve of the magnetic denſities and its extent in bars of a white temper, in thoſe brought back to the ſtate of ſpring, and in thoſe brought down to white temper. The object of this reſearch is very intereſting, either for conſtructing artificial magnets, or for fixing the ſtate and dimenſions of needles moſt advantageous for thoſe employed in the mariner's compaſs. The author found, for example, that when the bars are very long in regard to their other dimenſions, it is of advantage, after they are tempered almoſt to a white, to bring them back to blue: for the excellent cylindric wires of ſteel uſed in commerce, when magnetized with the ſtrongeſt magnets, have the ſame directive force whether they are tempered to a white or are brought back to the ſtate of blue ſpring when their length is thirty-three times their diameter; but if they are much longer than thirty-three times their diameter there is a great advantage in bringing them to the ſtate of ſpring; and on the other hand, when their length is much leſs than thirty-three times their diameter, there is a very great advantage in tempering them white. The cauſes of theſe varieties will be detailed in the ſecond part of this memoir; and it will there be ſeen, ſays the author, that they only confirm theory, which he has always compared with experience, in the different memoirs publiſhed in the volumes of the Academy of Sciences and thoſe of the Inſtitute.

Phyſics.

C. Cuvier, ſecretary, read the following notice:

On the Gas obtained from the Reduction of the Oxide of Zinc, and the Nature of Charcoal.

The aëriform fluid obtained by reducing the oxide of zinc by charcoal was ſtated by C. Guyton to be a gaſeous oxide of carbon, that is to ſay, carbonic acid with exceſs of baſe. Continuing to reaſon on this hypotheſis, Guyton thought that the ſubſtance found there in exceſs, being very much divided and weakly retained, ought to be very much diſpoſed to exerciſe its affinities, and that it could operate reductions cold and in the wet way. Trials, however, made with ſolutions of lead, mercury, and ſilver, did not anſwer his expectation. The gas experienced no alteration in theſe liquors; but when put in contact with oxygenated muriatic acid there was a ſpontaneous combuſtion, and the gas, converted into common carbonic acid by the acceſſion of the oxygen of the muriatic acid, became capable of precipitating lime water.

Berthollet had a peculiar opinion in regard to the manner in which this reduction of the oxide of zinc takes place, and which he explained by the compoſition of common charcoal, which contains a large quantity of hydrogen.

This doctrine he has endeavoured to eſtabliſh by a very extenſive diſſertation on the nature of charcoal, and the different products thence obtained.

For this purpoſe he reſumed all the labours of his predeceſſors on charcoal, and ſubjected them to a new examination. He has ſhown that Lavoiſier was not altogether exact in his analyſis of the carbonic acid, becauſe at that period he was not well acquainted with the ſpecific gravity of oxygen gas, and becauſe he did not pay attention to the water, of which the carbonic acid always contains a great deal. He brings forward the proofs which C. Monge gave of this laſt-mentioned fact, and has eſtabliſhed, that common charcoal contains a large quantity of hydrogen and a little oxygen. He has ſhown that this charcoal diſtilled in a pneumato-chemical apparatus gives carbonated hydrogen gas only as long as its oxygen is not exhauſted, becauſe a little of the latter ſubſtance is neceſſary to produce that gas, which conſequently is a ternary combination. The reſiduum then contains only carbon and a leſs quantity of hydrogen than ſimple heat can deprive it of; and the diamond, according to Berthollet, differs from calcined charcoal only by its not containing hydrogen, and not, as Guyton thought, becauſe the charcoal already contains oxygen.

In regard to the different combinations which contain hydrogen and carbon, Berthollet divides them into two claſſes: the ternary, which contain oxygen, alſo ſuch as the gas extricated from ſugar, that obtained from metallic oxides with charcoal, and that which carbonate of barytes produces with the ſame ſubſtance, &c.; and the binary, which contain no oxygen, as the gas known under the name of *olefying*, that extracted from alcohol and oil, and, in all probability, that ariſing from the decompoſition of water by charcoal. His method to determine whether gas contains oxygen or not, is ſimple and ingenious. It conſiſts in general in burning the gas, and obſerving whether the oxygen it has conſumed during its combuſtion has been ſufficient to produce the reſults. In the contrary caſe, the gas burnt muſt have already contained a greater or leſs quantity of it.

Berthollet has examined alſo the compoſition of the carbonic acid, not by a direct method, but by admitting as true the proportion which beſt correſponded to all the experiments which his reſearches required. 100 cubic inches of this acid contain, according to Berthollet, 84 cubic inches or 43 grains of oxygen, 16 grains of carbon, and from 9 to 10 grains of water.

In regard to the queſtion which gave riſe to all this labour, Berthollet conſiders the gas reſulting from the reduction of zinc, as a ternary compound of carbon, oxygen and hydrogen, in the proportion of two parts of hydrogen, ſeven parts of carbon, and thirty-two of oxygen.

While the French chemiſts were diſcuſſing this queſtion with ſo much activity, it engaged the attention of foreigners alſo. Mr. Cruickſhank gave to the gas obtained the ſame name as Guyton, viz. *oxide of carbon.* He however ſaw water formed in all his experiments except one, where, by the nature of the ſubſtances employed, no more ought to have been produced than juſt as much as the carbonic acid produced at the ſame time could abſorb: thus the reſults of the Engliſh chemiſt were, at bottom, the ſame as the laſt made by Berthollet, though he employed the denominations of Guyton.

XXXIII. *Intelligence and Miſcellaneous Articles.*

November 1801.

GALVANISM.

J. TOURDES, profeſſor in the School of Medicine at Straſburgh, has lately communicated the following notice on this ſubject to profeſſor Volta:—"The confidence and friendſhip of which you have ſo often given me proofs during my reſidence in Italy, embolden me to communicate to you the reſult of an experiment which appears to me to reſolve one of the moſt diſputed points of phyſiology, that of the vitality of the blood. This liquid deprived of the ſerum, lymph, &c. reduced to the fibrous part, and ſubjected to your galvanic or rather electric apparatus, (for the identity of the galvanic and electric fluids have been eſtabliſhed in an inconteſtable manner by your late reſearches) at the temperature of about 30 degrees of Reaumur, exhibited a trembling oſcillation and palpitation analogous to thoſe experienced by fleſh of animals newly killed; a *double motion of contraction and dilatation*, ſenſible to the eye by means of a magnifying glaſs; a characteriſtic mark of the vital force peculiar to the muſcles, cellular tiſſue," &c.

Since our laſt publication ſome intereſting experiments have been performed with the circuit of Volta that have given reſults which, though analogous to facts before known, were equally new and intereſting, and ſerve to prove that we are as yet only entering the threſhold of the inquiry reſpecting its powers, and the purpoſes to which it it may hereafter be applicable.

The experiments alluded to were performed by the Aſkeſian Society on the 4th inſtant, being their ſecond meeting for the preſent ſeſſion.

The battery conſiſted of two troughs, each containing 60 plates of ſilver, and an equal number of zinc; in all 120 pairs of metallic plates, each $2\frac{1}{4}$ inches ſquare, or containing $5\frac{1}{16}$ ſquare inches of ſurface.

By means of this battery the Society deflagrated gold with great facility. The gold in thin leaves was connected with one end of the battery, and, on being brought into contact with the exterior plate of the other end to complete the circuit, inſtantly inflamed. The combuſtion only went on at thoſe points in which the gold leaf and the plate were in contact, and ceaſed entirely unleſs the gold was kept advancing againſt the plate, to ſupply, by freſh portions brought into contact, the place of thoſe burnt away. It is ſingular that no oxide or reſidue of any kind could be diſcovered; and it is probable that the gold was volatilized by the ſame heat that occaſioned its deflagration: this point, however, is to be further inveſtigated, when a larger portion of gold will be expoſed to the action of the Voltaic circuit, to determine with certainty whether the whole, or any part of it, is converted into vapour.

In our number for September, we mentioned the combuſtion of phoſphorus by the Voltaic trough, which was effected by putting a ſmall piece on the point of an iron wire, and deflagrating the wire by bringing it in contact in the circuit. By ſimilar means the Society exploded gunpowder. A ſmall portion of it was diſpoſed in the folds of ſome gold leaf, which was then deflagrated in the manner already deſcribed, and inflamed the powder.

CHEMISTRY.

A new fact was mentioned at a late meeting of the Britiſh Mineralogical Society, by Mr. Sandman, one of the members, and an excellent chemiſt. Some months ſince, to one part of butter of antimony he added five parts of water: inſtead of an inſtant precipitation taking place, the mixture aſſumed the form of a thickiſh maſs, of ſuch a conſiſtency, that it would not even pour from the veſſel. After four months, the mixture began to clear itſelf, and threw down a quantity of very white ſmall cryſtals, ſome of which were laid before the Society. Their form could not be accurately determined, but ſeemed to approach the cubic. Whether they be a cryſtallized oxide, or merely a muriate of antimony, has not yet been aſcertained; but the queſtion is to be inveſtigated, and the experiment will be repeated on a large ſcale.

The ingenious Mr. Davy, of the Royal Inſtitution of Great Britain, has been occupied for ſome time in a ſeries of experiments upon tanning, which will ſoon be laid before the public. He has tried a number of woods and barks, beſides thoſe that have been hitherto thought excluſively adapted to the purpoſe, and has met with ſeveral that contain the tanning principle in great quantity. His experiments will lead to a great ſaving in the expenſe of even oak bark, which it has been uſual to have picked and ſeparated from one part of the rind, which is found to be, notwithſtanding, as rich in tan as any other part of the bark. One circumſtance remarked by this chemiſt is curious: Acorns do not ſeem to contain in their natural ſtate any portion of tan; but after being baked in a heat of about 212° F. they are found to contain a conſiderable quantity. If they be overbaked, they again loſe that quality.

The ſame chemiſt is engaged in analyſing the ink of the cuttle-fiſh. The experiments are not finiſhed, but from the progreſs he has made it appears to be carbon held in ſome ſtate of ſolution.

METEOROLOGY.

A phænomenon was ſeen to paſs Batan Rouge, on the Miſſiſippi, in the latitude of 31° north, on the night of the 5th of April 1800, of which the following is the beſt deſcription we have been able to obtain *:—It was firſt ſeen in the ſouth-weſt, and moved ſo rapidly, paſſing over the heads of the ſpectators, as to diſappear in the north-eaſt in about a quarter of a minute. Its apparent ſize was that of a large houſe ſeventy or eighty feet long, and of a form nearly reſembling the annexed figure (Plate V. fig. 3.)

It appeared to be about 200 yards above the ſurface of the earth, wholly luminous, but not emitting ſparks; of a colour reſembling the ſun near the horizon in a cold froſty evening, and which may be called a crimſon red. When paſſing right over the heads of the ſpectators, the light on the ſurface of the earth was little ſhort of the effect of ſun-beams, though at the ſame time, looking another way, the ſtars were viſible; which appears to be a confirmation of the opinion formed of its moderate elevation.

In paſſing, a conſiderable degree of heat was felt by thoſe who ſaw it, but no electric ſenſation. Immediately after it diſappeared in the north-eaſt, a violent ruſhing noiſe was heard, as if the phænomenon was bearing down the foreſt before it, and in a few ſeconds after a tremendous craſh, ſimilar to that of the largeſt piece of ordnance, cauſing at the ſame time a very ſenſible earthquake.

Search being afterwards made in the place where the burning body fell, every vegetable body was found burnt or greatly ſcorched, and a conſiderable portion of the ſurface of the earth broken up.

ASTRONOMY.

Profeſſor Bode, of Berlin, has received two letters from M. Piazzi of Palermo, in which that indefatigable obſerver of the heavens declares that he is now convinced, as profeſſor Bode is, that the ſtar diſcovered on the 13th of January laſt is a planet, though owing to the faintneſs of its light he at firſt thought otherwiſe. He adds alſo, that it will not be viſible till November †. The German aſtronomers propoſe to

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* Communicated in a letter from W. Dunbar, Eſq. (one of the gentlemen employed in running the line of demarcation between the United States of America and the Spaniſh ſettlements) to Mr. Swift, of Fenchurch-ſtreet.

† In England the general belief is that this ſuppoſed planet will turn out to be a comet.

call this new planet Juno, by analogy to the names of the other planets; but M. Piazzi wiſhes that it ſhould be called *Ceres Fernandia*, in alluſion to Sicily, which is conſecrated to Ceres, and to the name of the monarch who at preſent reigns over that country.

STATISTICS.

The French miniſter of the interior is now employed in collecting materials for a ſtatiſtic deſcription of France. An account of fifteen departments has already been received, ſome of which the miniſter has publiſhed. The names of the departments, reſpecting which memoirs have been received, are, Allier, Upper Alps, Aude, Cher, La Drôme, Ille-et-Vilaine, Mont-Blanc, Orne, Ourthe, Upper Saône, Two Sevres, Tarn, Var, Voſges, and La Vendée. Theſe memoirs, written by the prefects of the different departments, contain intereſting facts, and curious details, reſpecting the different diſtricts of France, and will form the firſt collection of poſitive information reſpecting the ſtate of that extenſive country.

EARTHQUAKE.

The following are the particulars of an earthquake which took place at Bologna, as inſcribed in the regiſters of the obſervatory, by C. Ciccolini:

This morning (October 8) at 8 hours 52 minutes 53 ſeconds, the air being calm, the ſky overcaſt, and Reaumur's thermometer at 13 0′, I felt three ſtrong ſhocks of an earthquake. The direction of the firſt was from north-eaſt to ſouth-weſt: the undulation gradually decreaſed, when a ſecond and a third ſhock were felt in the ſame direction as the firſt. Theſe three ſhocks were in general taken to be one; but I was able to diſtinguiſh them exactly, as they continued half a minute; one of the clocks in the obſervatory having ſtopped, I had the exact time of the earthquake. Some chimneys were thrown down in the city, one of which had almoſt beat in the ceiling of an apartment where the celebrated mathematician Soladini lodges. The remembrance of the earthquakes which afflicted the city of Bologna almoſt for a whole year, in 1779 and 780, occaſioned a general conſternation. It muſt, however, be obſerved, that the ſtate of the atmoſphere was very different from what it was at that period, the ſun being then pale, the ſky continually obſcured by clouds of a lead colour, and the horizon darkened by very thick fogs. Lightning frequently fell, and igneous meteors appeared under different forms; and there were above 80 auroræ boreales. Nothing of the kind took place during the preſent ſeaſon. The earthquake was felt with violence at Catene and Ceuta.

XXXIV. *On the chemical Knowledge which the Philoſophers of the 16th and 17th Centuries had of the different Gaſes*[*].

MODERN chemiſtry has been of the utmoſt ſervice to the ſcience of nature in general, by making us better acquainted with the finer ſubſtances of light, caloric, and, in particular, of the gaſes: but very imperfect would be our knowledge of nature, very poor would be our harveſt of new truths, and very inſignificant would be our progreſs in our reſearches reſpecting nature, if we had not reaſon, from the rapid advance which philoſophy has made in modern times, to flatter ourſelves with the hopes of a more accurate knowledge of theſe finer ſubſtances. We ſhould, however, be guilty of a very great error, if we aſcribed the whole merit of this more accurate knowledge to the 18th century; for, though moſt of the philoſophers of the two preceding centuries either overlooked theſe kinds of gas altogether, or conſidered them as common air corrupted and deprived of its elaſticity, yet ſome of them were more clear-ſighted, and penetrated deeper into theſe ſecrets of nature.

If we paſs over the remoteſt traces which occur here and there in the works of the antient writers, J. Baptiſt van Helmont ſeems to ſtand at the head of thoſe more acute-ſighted philoſophers; for, though it cannot be denied that he diſfigured his valuable diſcoveries by numerous fictions, and concealed them under new and commonly barbarous names, which were for the moſt part improper; yet he firſt informed phyſicians and naturaliſts, and proved clearly by obſervations and experiments, that, beſides the air which we continually breathe, there are other fluids, which, though they approach very near to the air in ſubtlety, tranſparency, and particularly in elaſticity, yet differ from it very much; and, as they differ alſo from vapours, he has diſtinguiſhed them by the particular name of *gas*[†]. He endeavoured to eſtabliſh the difference between them by this circumſtance—that as air nouriſhes flame, theſe fluids extinguiſh it[‡], and even the vital flame[§]. He was acquainted alſo with the air of the celebrated *Grotto del Cane*, near Naples[*], as well as with the exhalations in mines, againſt the pernicious effects of which cautions had been given long before his time by Andrew Libavius[†] and George Agricola[‡]; and he knew alſo that they killed animals which had purpoſely or accidentally been expoſed to them, as well as imprudent perſons, and particularly miners. He was in particular acquainted with that gas which ſome, becauſe it inflames when it comes in contact with a burning body, call inflammable gas, and which others, becauſe they conſider it as an eſſential component part of water, name hydrogen gas; and he knew alſo that it had been obſerved before him in mines by Libavius: he convinced himſelf that even eructations were of the ſame nature, and that it formed a principal component part of ſmoke: he likewiſe conſidered flame to be only inflamed ſmoke[§]. His knowledge, however, was not confined to this gas alone, for under the name of *gas ſylveſtre* he was acquainted with nitrous gas, produced by the action of aquafortis when ſilver is diſſolved in it[||]; and he knew alſo, that when it came in contact with the atmoſphere it formed fiery red vapours.

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[*] From *Gœttingiſches Journal der Naturwiſſenſchaften*, by J. F. Gmelin, vol. i. p. 4.

[†] Ortus Medicinæ; Amſterdam 1648. 4to. p. 73.

[‡] Ibid. p. 163.

[§] Ibid. p. 47. 87. 95. 110. 163.

He was well acquainted alſo with the muriatic acid gas; for what elſe could he underſtand by that gas into which, to uſe his own expreſſions[¶], ſal-ammoniac and aquafortis were converted when mixed together and made luke-warm? He knew likewiſe the ſulphurous acid gas; for he ſought in it, as a component part of the vapour of burning ſulphur, the reaſon why that vapour extinguiſhes a lighted candle or taper when expoſed to it.

He had become acquainted alſo, different ways, with that gas which ſhows itſelf in air in which various bodies, and particularly coals, have been burnt[**]; and on that account he had given it the name of coal gas; though he had ſeen abundance of it in various places, and accompanying other phænomena, both in living human bodies, as for example in belching[††], and on a larger ſcale, as in wine and beer cellars, and during the efferveſcence of different ſubſtances when acids were poured upon them[‡‡], in the *Grotto del Cane* and other ſubterranean places [*]; and particularly in acid waters, from which it riſes in bubbles, and they are indebted to it, he ſays, for all their healing qualities [†]. He remarks alſo of this gas [‡], that, as it extinguiſhes the flame of a lamp or taper, it extinguiſhes alſo the flame of life. He proved, by a very ſimple experiment [§], that the volume as well as the goodneſs of air is leſſened by bodies burning in it; for, having placed a burning taper under a glaſs in ſuch a manner that it ſtood three inches above the water in the veſſel, he ſaw the water riſe, aſſume the place of the decreaſing air, and at length extinguiſh the taper.

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[*] Ortus Medicinæ, p. 110. 163. 615.

[†] Commentat. Metallic. Francof. ad Mœn. 1597. 4to. p. 297.

[‡] De Natura eorum quæ effluunt ex terra.

[§] Commentat. Metallic. ut ſupra, p. 421. 414. 84.

[||] De Aquis Stygiis, ut ſupra, p. 615. 424.

[¶] Ortus Medicinæ, p. 423.

[**] Ibid. p. 110. 106. 110. 405, 406—437: Tumulus peſtis, p. 55.

[††] Ibid. p. 116. 421. 431.

[‡‡] Ibid. p. 424; and De Febribus, c. ix. p. 43.

He had obſerved alſo that an air could be produced from nitre, which he called *flame gas*, and which was diſengaged on coal being added to it [||]; and he thence conjectured the preſence of vital air in that ſalt. He entertained alſo the opinion, which modern chemiſtry has ſupported by ſo many concluſive proofs, that all theſe air-like fluids [¶] are indebted for their form to the effects of fire, or, as we commonly ſay at preſent, to caloric.

It cannot, however, be denied, that Van Helmont conſidered as different gaſes thoſe which differ only by accidental corruption, as the carbonic acid gas, according as it is drawn from this or that body, and in this or that manner; and that he confounded others, or did not make a proper diſtinction between them. Yet before Prieſtley, moſt of the philoſophers who paid attention to this ſubject, made no other difference between theſe kinds of gas, than that ſome of them inflame when brought into contact with a burning body, while others inſtantly extinguiſh a flame.

Van Helmont ſuſpected alſo, as appears clearly from his own words, the great ſimilarity which, in regard to air, is found between flame and animal life. This was more clearly perceived by Thomas Willis [**] and Francis Sylvius de le Boë [††], a man whom few have equalled in genius or eloquence; for the latter ſays, that, as flame requires more air than the quietly glowing fire of coal, in the like manner breathing animals which breathe through real lungs have need of more air than thoſe which merely evaporate, that is to ſay, than ſuch was have no evident and real lungs; and as, in general, a ſtronger fire requires a ſtronger draught of air than a weaker, in the like manner a more active life requires quicker breathing, and a more ſluggiſh a ſlower: as air acts differently on flame according as it is more or leſs clear, warm, or dry, in the like manner it acts upon breathing and the vital flame: and, in the laſt place, as flame burns more briſkly according as it has free acceſs to the air on all ſides, and is weaker when the air is withheld, and is at length totally extinguiſhed, breathing is expoſed to the ſame effects under the like circumſtances.

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[*] Ortus Medicinæ.

[†] De Lithiaſi, c. iv. p. 34.

[‡] Ortus Medicinæ, p. 163.

[§] Ibid. p. 84.

[||] Ibid. p. 423.

[¶] Ibid. p. 72.

[**] Affectionum quæ dicuntur hyſtericæ et hypochondriacæ Pathologia ſpaſmodica, contra Reſponſionem epiſtolarem Nath. Highmori, cui acceſſerunt Exercitationes medico-phyſicæ duæ, &c.; Lugd. Bat. 1671. 12mo. p. 80—102.

[††] Diſputat. de Reſpiratione, § 69—73.

This Sylvius proved nothing by experiments, but he made it probable, according to his manner, that ſouriſh particles of nitre [*] floated about in the common atmoſphere; and approached ſo near to the prevailing principles of the preſent day [†], that he ſuppoſed fire (or caloric) was continually diffuſed through the air.

The celebrated Spaniſh mineralogiſt Alphonſo Barba was acquainted with thoſe pernicious damps which ariſe ſo often in mines, and which, though they reſemble air in other reſpects, have however an offenſive ſmell, extinguiſh lights, and deprive men, birds, and even ſnakes, of their life [‡].

Boyle, to whom natural philoſophy in general is under ſo many obligations, aſcribed that increaſe of weight which metals acquire by calcination [§], as he had frequently obſerved, rather to the fire than to particles from the atmoſphere, and was therefore very remote from that opinion which prevails in the preſent century, and which is ſupported by ſo many experiments; but he was at the ſame time acquainted with the carbonic acid gas as it riſes from coral, when it effervеſces with vinegar, from bread, cherries, grapes, pears, apricots, plums, gooſeberries, peas, &c. when they ferment [‖], and its highly pernicious effects on animals; as alſo with the inflammable, as it partly occurs in abundance in many mines, for example, thoſe of Hungary [¶], partly as it ariſes by a ſolution of iron in diluted ſulphuric acid or muriatic acid [**], and its property of inflaming when brought into contact with flame.

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[*] Praxis Medicinæ, lib. i. c. 21. § 111.

[†] Ibid. lib. iii. c. 11. § 47.

[‡] Bergbüchlein, Frankf. 1726. part i. c. 2. p. 5—8.

[§] Detect. penetrabilit. vitr. p. 296, 297.

[‖] Second continuation of Phyſico-mechanical Experiments, art. ii. exp. 11. art. iii. exp. 1. 7. 10, 11. art. v. exp. 1, 2. 13.

[¶] Examinat. of Antiperiſtaſis, Oper. b. ii. p. 366.

[**] Nova Experimenta Phyſico-mechanica de Vi Aëris elaſtica, p. 152—154: New Experiments touching the Relation betwixt Flame and Air, Oper. b. iii. p. 255, 256.

The latter as a highly pernicious kind of gas, abundant in coal-pits, is mentioned by Martin Liſter, Jeſſop, and P. R. Moſlyn; and a fire which took place in a coal-pit by the inflammation of ſuch vapour is mentioned by Hodgſon [*]; a like vapour was found by Beaumont in other ſubterranean holes [†]; by T. Shirley [‡] above a ſpring, and Wolfſtriegel and Vollgnad [§] in a ſpring. Pope, in the firſt volume of the Philoſophical Tranſactions, gave a very lively picture of that corrupted kind of gas in a hole near the lake of Agnano; and L. A. Portius [‖] and Leonhard à Capua [¶], that of the *Grotto del Cane* and other caverns in the neighbourhood of Naples; E. Hagendorn [**] and F. Hoffmann, the dangerous quality of air in which coals have been burnt [††]; and T. Birch, that of air which has been corrupted by the breath of animals [‡‡]. S. Ledelius mentions the death of a perſon which took place in a cellar filled with wine in a ſtate of fermentation [§§]. This gas, as it riſes both from fermenting liquids as well as from lixivious ſalts and earths, when they effervesce with acids, and in which, even at his time, Bernoulli ſought for the cauſe of this effervefcing [‖‖], was perfectly well known to ſir Chriſtopher Wren [¶¶]. He relates a method by which ſuch fluids can be collected and preſerved in veſſels; and he knew that the above gas is abſorbed by water, and he diſtinguiſhes it very clearly from nitrous gas. Even the nitrous gas of the moderns ſeems to have been known to Huygens and Papin, for they relate [***] that they obtained ſuch a fluid from the mixture of ſpirit of wine and ſpirit of nitre under the receiver of an air-pump.

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[*] Philoſophical Tranſactions, vol. x. 1675, no. 117. p. 391, &c. Ibid. no. 119. p. 450. Ibid. vol. xii. 1677. no. 136. p. 895. Ibid. vol. xi. 1676. no. 134.

[†] Hooke's Philoſophical Collections, 1679. 4to. no. 1.

[‡] Philoſophical Tranſactions, vol. ii. 1667. no. 25. p. 482.

[§] Miſcellan. Acad. Cæſ. Natur. Curioſ 4to. dec. i. Lipſ. 1670. obſervat. xxxiii. an. 4 & 5. Francof. & Lipſ. 1676. obſervat. clxxi.

[‖] Diſſertation. Variar. Venet. 1683. ii.

[¶] Lezioni intorno alla Natura delle Mofete; Napol. 1683. 4to.

[**] Obſervat. et Hiſtoriar. Medico-practicar. rarior. Centur. tres; Rudolſt. & Goerliz. 1698. 8vo.

[††] Bedenken von dem todlichen Dampfe der Holzkohlen; Halle 1716. 8vo.

[‡‡] Hiſtory of the Royal Society of London for the Improvement of Natural Knowledge; London, 4to. vol. ii. 1668.

[§§] Ephemerid. Acad. Cæſar. Nat. Curioſ. dec. iii. an. 2. obſ. 45.

[‖‖] Diſſertatio de Efferveſ. et Fermentatione; Baſil. 1690. 4to.

[¶¶] Philoſophical Tranſactions, vol. x. 1675. p. 446.

[***] Ibid.

At the ſame period, F. Slare and T. Willis aſcribed [*] the dark red colour of the blood to the air; and J. Mayow [†], another Engliſhman, with whom another Oxford phyſician, Henry Mund [‡], and alſo Willis [§], L. M. Barbieri, and J. B. Giovanni [||], concurred, made the whole uſe of breathing to conſiſt in this—that the lungs of animals inhale from the atmoſphere nitrous particles, which are diffuſed over the animal ſpirits and communicate warmth to the blood. J. N. Pechlin [¶], alſo, attributes the faculty of ſome divers being able to remain longer under water to a greater quantity of nitre. From all this it appears that the phyſicians of that period had a kind of idea of vital air, and its influence on the animal œconomy.

Stephen Hales, who made further progreſs by his numerous experiments in diſcovering the ſecrets of nature, placed beyond all doubt, by a long ſeries of experiments, the elaſticity of theſe fluids as they are expelled from bodies by heat, fermentation, corruption, and effervefcence, a power which was before obſerved by Newton [**], and at the ſame time ſhowed ſeveral and ingenious methods [††] how they could be obtained, preſerved, meaſured, weighed, and even handled. He remarked the inflammability of thoſe which the inflammable bodies of every kingdom of nature yield by expoſure to a ſtrong heat, the properties of others which ariſe from effervefcing mixtures, and which ſuddenly extinguiſh flame. He obſerved likewiſe, exceedingly well, the great difference of the nitrous gas which he obtained when he poured upon antimony *aqua regia*, or ſpirit of nitre; or the latter diluted with water [aquafortis] upon iron filings or quickſilver, and its property of forming red clouds with common air as ſoon as it comes in contact with it; and, by repeated experiments, that it abſorbed a great portion of air; and alſo, that the oftener the experiment was repeated, the gas always abſorbed the leſs air; that ſeveral of theſe gaſes were abſorbed by water; how much the beſt air is corrupted by the breathing of even the ſoundeſt men, ſo that at laſt it is totally unfit for reſpiration. He knew alſo the ammoniacal as well as the muriatic acid gas, and the ſulphuric acid gas, and had learned, from his own experience, that the latter can be as ſtrongly compreſſed as common air. He knew alſo that metals increaſe in weight by calcination, and again decreaſe on being revived; for he found that red lead in the preparation had increaſed in weight about a twentieth part, and by a ſtrong heat gave a great deal of air: his own experience had taught him alſo that phoſphorus, ſulphur, and a tallow candle, abſorb ſome of the air in which they burn, as animals abſorb ſome of the air in which they breathe; though he aſcribed the pernicious change which the air thereby experiences, not to the loſs it ſuſtains, but rather to the corrupt evaporation with which it is filled. He had obſerved, though leſs perfectly, that phoſphorus after combuſtion increaſed in weight by imbibing ſomething from the atmoſphere. He had diſcovered that an aëriform ſubſtance was contained in acidulous waters, and that air was continually abſorbed by plants in a healthy ſtate*.

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[*] Philoſophical Tranſactions for 1682, no. 204. Ibid. p. 92—94.

[†] Tractatus quinque Medico-phyſici; Oxon. 1699. 8vo. no. 1,

[‡] Βιοχρηστολογια; Oxon. 1680. 8vo.

[§] Philoſoph. Tranſact. ut ſupra.

[||] Spiritus Nitro Aërii Operationes in Microcoſmum; Bonon. 1681. 12mo. Diſſertation, ſur la Fermentation ſur le Nitre et ſur l'Air; Touloufe 1685. 12mo.

[¶] De Aëris et Alimenti Defectu; Kilon. 1676. 8vo.

[**] Optics; London 1701. 4to.

[††] Statical Eſſays; London, 8vo. third edit. 1783. vol. i. c. 6. and vol. ii. p. 315. 317.

In this difficult doctrine, however, he left a great deal to be explained by his followers; for he did not define and was not acquainted with the difference of many of the gaſes, and ſome of them eſcaped his notice altogether. The further illuſtration of them was reſerved for the modern chemiſts. Thus, in particular, the following authors have very much contributed to enlarge our knowledge of the carbonic acid:—Dr. Black in Medical and Philoſophical Commentaries, by a Society in Edinburgh, vol. ii.; Dr. Macbride in Experimental Eſſays on Medical and Philoſophical Subjects, London 1764. 8vo; T. Henry in his Experiments and Obſervations, London 1773. 8vo.; T. And. Emmer in *Diſſertatio de Aëre fixo ſeu Acido aëreo*, Edin. 1784; and the Dutch naturaliſt D. De Smedt in *Diſſertatio de Aëre fixo*, Ultraj. 1773. 8vo. Among the Germans, N. J. Jacquin in *Examen Chemicum Doctrinæ Meyerianæ de Acido pingui, et Blackianæ de Aëre fixo reſpectu Calcis*, Vindob. 1769. 8vo; and the late J. J. Well, in his Defence of Dr. Black's Doctrine reſpecting fixed Air, againſt the Objections made by Wiegleb, Vienna 1771. 8vo.; and on The Cauſes of the heating of unſlaked Lime, Vienna 1772. 8vo. Among the Italians, F. F. Fontana in his Phyſical Reſearches reſpecting fixed Air, Florence 1774. 8vo. In Swiſſerland, Sol. Schinz in *De Aëre, ejus Speciebus, præcipue de Aëre fixo Lapidis calcarei*, Turic. 1778. 4to.; and in Sweden, T. Bergman in

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* Statical Eſſays paſſim.

De Acido aërio Opuſcula Phyſica et Chemica, vol. i. p. 1, &c. The laſt-mentioned chemiſt ſhowed alſo, in his treatiſe *De Analyſi Aquarum* in his *Opuſcula*, vol. i. p. 68, as did in England, Brownrigg in the Philoſophical Tranſactions, vol. lxiv. part 2. no. 39; and, in France, Venel in *Memoires preſentés à l'Academie des Sciences à Paris par divers Savans*, vol. ii. that it is to this acid that acid waters are principally indebted for their properties; ſo that ſeveral chemical authors, ſuch as Bergman *De Aquis medicatis frigidis arte parandis*, in his *Opuſcula*, vol. i. art. 6. p. 186; Venel in the work above quoted; Rouelle in Roux's *Journal de Medecine, Chirurgie*, &c. for May 1774; Duchanoy *Eſſai d'imiter les Eaux Minerales*, Paris 1780. 8vo.; Laugier *Mineralogie nouvelle, ou l'Art de faire les Eaux Minerales*, Sens 1786. 8vo.; Meyer, apothecary to the court at Stettin, in the Tranſactions of the Friendly Society of the Searchers into Nature of Berlin, vol. iv. 1781. p. 313; and the late profeſſor Köſtlin in his Method of imitating the Water of Acid Springs by Means of fixed Air, Stuttgard 1780. 8vo., have deſcribed the proceſs by which they can be exceedingly well imitated.

With the like care and ingenuity the following writers have examined the nature of the inflammable kinds of gas:—Volta *Lettere ſull' Aria inflammabile delle Paludi*, Milan 1777; Senebier *Recherches analytiques ſur la Nature de l'Air inflammable*, Geneva 1784. 8vo.; Minkelers *Memoire ſur l'Air inflammable de differentes Subſtances*, Louvain 1784. 8vo.; Moſcheni *Eſame intorno alla Natura e Proprieta dell' Aria inflammabile paludoſa*, Lucca 1788; Laſſone in *Memoires de l'Academie des Sciences à Paris* for the year 1766; Kirwan, Philoſophical Tranſactions, vol. lxxvi. 1786. part i. Gengembre in *Memoires preſentés à l'Academie des Sciences par divers Savans Etrangers*, vol. x.; Raymond *Annales de Chimie*, vol. x. 1791. p. 19; Nieuwland, Deiman, Trooſtwyk, and Bondt, *Recherches Phyſico-chymiques*, Amſterd. 8vo. 1792. no. 1. The three laſt, together with Laurenburg, examined the different kinds of inflammable gas. See Crell's Chemical Annals 1795. vol. ii. p. 195. 310. 430; and 1796. vol. ii. p. 110 and 222.

Reſpecting the common nitrous gas, we have the following works:—Fontana *Recherches phyſiques ſur la Nature de l'Air nitreux et de l'Air dephogiſtique*, Paris 1776. 8vo.; count Morozzo *Lettre ſur la Compoſition du Gas mephitique et du Gas nitreux*, Turin 1784. 8vo.; reſpecting Prieſtley's ſo called dephlogiſticated nitrous air, Bochaute in *Memoires de l'Academie des Sciences à Bruxelles* 1783; and Deiman, Bondt, Nieuwland, and Trooſtwyk, *Recherches Phyſico-chymiques*,

miques, no. 2. 1793; reſpecting azote, profeſſor Schmidt in Gren's Journal of Natural Philoſophy, vol. i. no. 3; and Wiegleb in Crell's Chemical Annals, 1796, vol. ii. p. 467; reſpecting the inflammable muriatic acid gas, Weſtrumb in Crell's Annals 1790, vol. i. p. 3. 100.

But this new field in the province of natural knowledge has been cultivated in a more extenſive manner by the following authors:—In Italy, Barbarigo in his *Saggi Fiſici*, Padova 1779. 8vo.; F. Fontana in *Memorie di Matematica e Fiſica della Societa Italiana*, Verona, 4to. vol. i. 1782; Bucci *Oſſervazioni circa il Flogiſto e le differente Specie d'Aria ſecondo le moderne Scoperte*, Pavia 1783. 8vo.; and Volta in *Propoſizioni ed Eſperienze di Aërologia*, Como 1776. In France, by Berthollet in *Obſervations ſur l'Air*, Paris 1776. 12mo.; Bucquet in *Mémoires préſentés à l'Académie des Sciences à Paris par divers Savans*, vol. vii.; Corrinus *Diſſert. ſiſtens Hiſtoriam Aëris factitii*, Argentor. 1776. 4to.; De la Metherie *Eſſai analytique ſur l'Air pur et les différentes Eſpèces d'Air*, Paris 1785. 8vo.; Rouland *Tableau hiſtorique des Propriétés et des Phénomènes de l'Air, conſidéré dans ſes differens Etats et ſous ſes divers Rapports*, Paris 1784. 8vo.; Sigaud de la Fond *Eſſai ſur differentes Eſpèces d'Air, qu'on deſigne ſous le Nom d'Air fixe*, Paris 1778. 8vo.; and Thouvenel *Mémoire Chimique et Medicinal ſur la Nature, les Uſages, et les Effêts de l'Air, et des Airs, des Alimens, et des Medicamens, relativement à l'Economie Animale, Ouvrage couronné par l'Academie de Toulouſe* 1778, Paris, 4to. In England, the anonymous author of a Treatiſe on the various Kinds of permanently elaſtic Fluids or Gaſes, 1777. 8vo; P. Plunket *Diſſertat. de Aëre Mephitico*, Edin. 1779; Cavallo on the Nature and Properties of Air; Cavendiſh in the Philoſophical Tranſactions, vol. lvi; and Higgins's Experiments and Obſervations relating to acetous Acid, fixable Air, denſe inflammable Air, &c. London 1786. 8vo. In the Netherlands, P. Von Trooſtwyk and Deiman, *Antw. op de Vraage: Wielke zyn de waarlyck onderſcheidene Soorten der luchtgelykende Vloeiſtoffen*, &c. Haarlem 1786. 8vo. And in Germany, Achard in *Nouveaux Mémoires de l'Acad. des Sciences á Berlin*, 1781; Herbert *De Aëre Fluidiſque ad Aëris Genus pertinentibus*, Vienn. 1780. 8vo.; Leonhardi *Program. Aërologiæ phyſico-chemicæ recentioris primæ Lineæ*, Lipſ. 1781; and Weber *Ueber gemeine und durch Auflöſung aus Körpern ëntwickelte Luft*, Landſhut 1785. 8vo.

But among the philoſophers who have endeavoured to explain and illuſtrate the nature of theſe gaſes by numerous experiments, made with great care, and accurately deſcribed, none have diſtinguiſhed themſelves more than Scheele and Dr. Prieſtley. The former a native of Germany, but ſettled in Sweden, invented very ſimple proceſſes, yet well calculated to anſwer the propoſed end, for examining theſe gaſes, by which means he diſcovered new kinds, and proved the exiſtence of thoſe already known in a much clearer method than had been done before in his *Abhandlung von der Luſt und dem Feuer nebſt einem Vorbericht von T. Bergman*, Upſal and Leipſic 1777. 8vo.; and in *Kongl. Svenſk. Vetenſkap. Academ. Handling*, 1774. p. 84. The latter, being furniſhed with a more extenſive apparatus, placed in a clearer point of view, not only the nature and differences of the gaſes before diſcovered, but diſcovered new ones alſo. An account of his experiments may be ſeen in Experiments and Obſervations on the different Kinds of Air, London, 8vo. vol. i. 1774, vol. ii. 1775, vol. iii. 1777; Experiments and Obſervations relating to various Branches of Natural Philoſophy, with a Continuation of Obſervations on Air, 8vo. vol. i. London 1779, vol. ii. Birmingham 1782, vol. iii. 1790; Directions for impregnating Water with fixed Air, &c. London 1772, 8vo.; Philoſophical Tranſactions of the American Society 1796.

XXXV. *An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr.* VOLTA. *By Mr.* HUMPHRY DAVY, *Lecturer on Chemiſtry in the Royal Inſtitution**.

I. ALL the galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore deſcribed by experimentaliſts, conſiſt (as far as my knowledge extends) of ſeries containing at leaſt two metallic ſubſtances, or one metal and charcoal, and a ſtratum of fluid. And it has been generally ſuppoſed that their agencies are, in ſome meaſure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of galvanic influence, exactly ſimilar to the accumulation in the common pile, may be produced by the arrangement of ſingle metallic plates, or arcs, with different ſtrata of fluids.

The train of reaſoning which led to the diſcovery of this fact, was produced by the obſervation of ſome phænomena relating to the connection of chemical changes with the evolution of galvanic power.

* From *Tranſactions of the Royal Society of London for* 1801.

It appeared, in ſeveral experiments, that ſeries of double metallic plates, incapable of acting as galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce galvanic effects, by being alternated with acids, or other fluids capable of oxidating one only of the metals of the ſeries. Thus, double plates, compoſed of ſilver and gold, (metals which have been ſuppoſed to differ very little in their powers of conducting electricity,) produced galvanic action, when placed in contact, in the common order, with cloths moiſtened in diluted nitric acid. And copper and ſilver acted powerfully with nitrate of mercury.

Theſe facts induced me to ſuppoſe, that the alternation of two metallic bodies with fluids, was eſſential to the production of accumulated galvanic influence, only ſo far as it furniſhed two conducting ſurfaces of different degrees of oxidability; and that this production would take place, if ſingle metallic plates could be connected together by different fluids in ſuch a manner, that one of their ſurfaces only ſhould undergo oxidation, the arrangement being regular.

On this ſuppoſition, I made a number of experiments on different arrangements of ſingle metals and fluids; and, after many various proceſſes, I was enabled to aſcertain, that many of theſe arrangements could be made active, not only when oxidations, but likewiſe when other chemical changes were going on in ſome of their parts.

In deſcribing the different galvanic combinations formed by ſingle metallic plates and fluids, I ſhall divide them into three claſſes, following, in the arrangement, the order of time with regard to diſcovery.

II. The firſt and moſt feeble claſs is compoſed, whenever ſingle metallic plates, or arcs, are arranged in ſuch a manner, that two of their ſurfaces, or ends oppoſite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this caſe, if the ſeries are numerous, and in regular alternation, galvanic influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and ſome other eaſily oxidable metals, act moſt powerfully in this claſs of combinations.

If pieces of poliſhed tin, about an inch ſquare and $\frac{1}{20}$ of an inch thick, be connected with woollen cloths of the ſame ſize, (moiſtened, ſome in water, and ſome in diluted nitrous acid,) in the following order—tin, acid, water, and ſo on, till twenty ſeries are put together—a feeble galvanic battery will be formed, capable of acting weakly on the organs of ſenſe, and of ſlowly producing the common appearances in water; the wire from the oxidating ſurface of the plates evolving hydrogen, and the wire from the non-oxidating ſurface (when of ſilver) depoſiting oxide.

In all caſes, when the batteries of the firſt claſs are erected perpendicularly, the cloth moiſtened in acid muſt be placed under the cloth moiſtened in water; and, in this arrangement, as the acid is ſpecifically heavier than water, little or no mixture of the fluids will take place.

When zinc is employed, on account of its rapid oxidation in water containing atmoſpheric air, three cloths ſhould be uſed; the firſt moiſtened in weak ſolution of ſulphuret of potaſh, (which is poſſeſſed of no power of action upon zinc, and which prevents it from acting upon the water;) the ſecond moiſtened in a ſolution of ſulphate of potaſh, of greater ſpecific gravity than the ſolution of ſulphuret; and the third wetted in an oxidating fluid ſpecifically heavier than either of the ſolutions. In this caſe, if the order be as follows—zinc, oxidating ſolution, ſolution of ſulphate of potaſh, ſolution of ſulphuret of potaſh—very little mixture of the fluids, or chemical action between them, will take place; and an alternation of twelve ſeries of this kind forms a battery capable of producing ſenſible effects.

III. The ſecond claſs of galvanic combinations with ſingle plates is formed, when plates, or arcs, compoſed of a metallic ſubſtance capable of acting upon ſulphurated hydrogen, or upon ſulphurets diſſolved in water, are formed into ſeries, with portions of a ſolution of ſulphuret of potaſh and water, in ſuch a manner, that one ſide of every plate, or arc, is in contact with water, whilſt the oppoſite ſide is acted on by the ſolution of ſulphuret. Under theſe circumſtances, when the alternation is regular, and the number of ſeries ſufficiently great, galvanic power is evolved; and water, placed in the circuit with ſilver wires, is acted on; oxide being depoſited on the wire connected with the ſide of the plate undergoing chemical alteration, whilſt hydrogen is evolved from the ſide in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of thoſe metals may be arranged with cloths, (moiſtened, ſome in water, and others in ſolution of ſulphuret of potaſh,) in the following order—metal, cloth moiſtened in ſulphuret of potaſh, cloth moiſtened in water, and ſo on.

Eight ſeries will produce ſenſible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this claſs of batteries, than ſilver; and ſilver more active than lead.

IV. The third and moſt powerful claſs of galvanic batteries, conſtructed with fluids and ſingle metals, is formed when metallic ſubſtances oxidable in acids, and capable of acting on ſolutions of ſulphurets, are connected, as plates, with oxidating fluids and ſolutions of ſulphuret of potaſh, in ſuch a manner, that the oppoſite ſides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The ſame metals that act in the ſecond claſs may be uſed in the third claſs; and the order of their powers is ſimilar. The pile may be erected in the ſame manner as the pile with zinc in the firſt claſs; the cloths moiſtened in acid being ſeparated from thoſe moiſtened in ſolution of ſulphuret by a third cloth ſoaked in ſolution of ſulphate of potaſh.

Three plates of copper or ſilver, arranged in this manner, in the juſt order, produce ſenſible effects; and twelve or thirteen ſeries are capable of giving weak ſhocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the apparatus evolving hydrogen; and the wire attached to the end acting on the ſulphuret, depoſiting oxide when compoſed of ſilver, and generating oxygen when of gold.

V. In all the ſingle metallic piles conſtructed with cloths, the action is very tranſient: the decompoſition of the acids, and of the ſulphurets, is generally completed in a few minutes; and, in conſequence, the galvanic influence ceaſes to be evolved. The arrangement of all the different ſeries may, however, (by means of an apparatus conſtructed after the ideas of count Rumford,) be made in ſuch a manner as to give conſiderable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and compoſed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for compoſing the ſeries. One half of theſe plates muſt be compoſed of horn or glaſs, and the other half of metallic ſubſtances; and the conductors of electricity, and the non-conductors, muſt be alternately cemented into the grooves, ſo as to form water-tight cells.

When the apparatus is uſed, theſe cells are filled, in the galvanic order, with different ſolutions, according to the claſs of the combination; and connected in pairs with each other, by ſlips of moiſtened cloth, carried over the non-conducting plates.

A combination of fifty copperplates, arranged in this manner,

ner, with weak ſolutions of nitrous acid, or nitrate of ammoniac, and ſulphuret of potaſh, gives pretty ſtrong ſhocks, rapidly evolves gas from water, and affects the condenſing electrometer.

It does not loſe its power of action for many hours; and, when this power is loſt, it may be reſtored by the addition of ſmall quantities of concentrated ſolutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and ſilver, it would appear, that the ſingle metallic batteries act equally well, when the metals made uſe of are ſlightly alloyed, and when they are in a ſtate of purity.

XXXVI. *Experiments on the chemical Production and Agency of Electricity.* *By* WILLIAM HYDE WOLLASTON, *M. D. F. R. S.*[*]

NOTWITHSTANDING the power of Mr. Volta's electric pile is now known to be proportional to the diſpoſition of one of the metals to be oxidated by the fluid interpoſed, a doubt has been entertained by many perſons, whether this power ariſes from the chemical action of the fluid on the metal; or, on the contrary, whether the oxidation itſelf may not be occaſioned by electricity, ſet in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cauſe of the electric phænomena obſerved, is, I think, to be inferred from the following experiments, which exhibit the galvanic proceſs reduced to its moſt ſimple ſtate.

Exper. 1. If a piece of zinc and a piece of ſilver have each one extremity immerſed in the ſame veſſel, containing ſulphuric or muriatic acid diluted with a large quantity of water, the zinc is diſſolved, and yields hydrogen gas, by decompoſition of the water: the ſilver, not being acted upon, has no power of decompoſing water; but, whenever the zinc and ſilver are made to touch, or any metallic communication is made between them, hydrogen gas is alſo formed at the ſurface of the ſilver.

Any other metal beſide zinc, which by aſſiſtance of the acid employed is capable of decompoſing water, will ſucceed equally, if the other wire conſiſts of a metal on which the acid has no effect.

Exper. 2. If zinc, iron, or copper, are employed with gold, in dilute nitric acid, nitrous gas is formed, in the ſame manner, and under the ſame circumſtances, as the hydrogen gas in the former experiment.

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[*] From the ſame.

Exper. 3. Experiments analogous to the former, and equally ſimple, may alſo be made with many metallic ſolutions. If, for inſtance, the ſolution contains copper, it will be precipitated by a piece of iron, and appear on its ſurface. Upon ſilver merely immerſed in the ſame ſolution, no ſuch effect is produced; but as ſoon as the two metals are brought into contact, the ſilver receives a coating of copper.

In the explanation of theſe experiments, it is neceſſary to advert to a point eſtabliſhed by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is ſufficient to oxidate one of the wires of communication, the wire connected with the oppoſite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this inſtance, is ſeen to depend on electricity, it is probable that, in other inſtances, electricity may be alſo requiſite for its converſion into gas. It would appear, therefore, that in the ſolution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that caſe, depends on a tranſition of electricity between the fluid and the metal.

We ſee, moreover, in the firſt experiment, that the zinc, without contact of any other metal, has the power of decompoſing water; and we can have no reaſon to ſuppoſe that the contact of the ſilver produces any new power, but that it ſerves merely as a conductor of electricity, and thereby occaſions the formation of hydrogen gas.

In the third experiment alſo, the iron by itſelf has the power of precipitating copper by means, I preſume, of electricity evolved during its ſolution; and here likewiſe the ſilver, by conducting that electricity, acquires the power of precipitating the copper in its metallic ſtate.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be ſeen that the ſame transfer of chemical power, and the ſame apparent reverſion of the uſual order of chemical affinities in the precipitation of copper by ſilver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, conſiſts of a cylinder 7 inches in diameter, with a conductor on each ſide 16 inches long and $3\frac{1}{2}$ inches diameter,

meter, each furniſhed with a ſliding electrometer to regulate the ſtrength of the ſpark received from them.

Exper. 4. Having a wire of fine ſilver, $\frac{1}{120}$ of an inch in diameter, I coated the middle of it, for 2 or 3 inches, with ſealing-wax, and, by cutting through in the middle of the wax, expoſed a ſection of the wire. The two coated extremities of the wire, thus divided, were immerſed in a ſolution of ſulphate of copper placed in an electric circuit between the two conductors; and ſparks, taken at $\frac{1}{10}$ of an inch diſtance, were paſſed by means of them through the ſolution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor had a precipitate formed on its ſurface, which, upon being burniſhed, was evidently copper; but the oppoſite wire had no ſuch coating.

Upon reverſing the direction of the current of electricity, the order of the phænomena was of courſe reverſed; the copper being ſhortly rediſſolved by aſſiſtance of the oxidating power of poſitive electricity, and a ſimilar precipitate formed on the oppoſite wire.

Exper. 5. A ſimilar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a ſolution of corroſive ſublimate, had the ſame ſucceſs.

The chemical agency, therefore, of common electricity is thus proved to be the ſame with the power excited by chemical means; but, ſince a difference has been obſerved in the comparative facility with which the pile of Volta decompoſes water, and produces other effects of oxidation and deoxidation of bodies expoſed to its action, I have been at ſome pains to remove this difficulty, and can at leaſt produce a very cloſe imitation of the galvanic phænomena by common electricity.

It has been thought neceſſary to employ powerful machines and large Leyden jars for the decompoſition of water; but, when I conſidered that the decompoſition muſt depend on duly proportioning the ſtrength of the charge of electricity to the quantity of water, and that the quantity expoſed to its action at the ſurface of communication depends on the extent of that ſurface, I hoped that, by reducing the ſurface of communication, the decompoſition of water might be effected by ſmaller machines, and with leſs powerful excitation, than have hitherto been uſed for that purpoſe; and in this hope I have not been diſappointed.

Exper. 6. Having procured a ſmall wire of fine gold, and given it as fine a point as I could, I inſerted it into a capillary glaſs tube; and, after heating the tube ſo as to make it adhere to the point, and cover it in every part, I gradually ground it down, till, with a pocket lens, I could diſcern that the point of the gold was expoſed.

The ſucceſs of this method exceeding my expectations, I coated ſeveral wires in the ſame manner, and found, that when ſparks from the conductors before mentioned were made to paſs through water, by means of a point ſo guarded, a ſpark paſſing to the diſtance of $\frac{1}{8}$ of an inch would decompoſe water, when the point expoſed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I eſtimated at $\frac{1}{1500}$, a ſucceſſion of ſparks, $\frac{1}{20}$ of an inch in length, afforded a current of ſmall bubbles of air.

I have ſince found that the ſame apparatus will decompoſe water with a wire $\frac{1}{10}$ of an inch diameter, coated in the manner before deſcribed, if the ſpark from the prime conductor paſſes to the diſtance of $\frac{1}{4}$ of an inch of air.

Exper. 7. In order to try how far the ſtrength of the electric ſpark might be reduced by proportional diminution of the extremity of the wire, I paſſed a ſolution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold lining the inner ſurface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the ſubſtance of the glaſs.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occaſion a ſtream of very ſmall bubbles to riſe from the extremity of the gold, although the wire, by which it communicated with the poſitive or negative conductor, was placed in abſolute contact with them. Hence it appears that decompoſition of water may take place by common electricity as well as by the electric pile, although no diſcernible ſparks are produced.

The appearance of two currents of air may alſo be imitated by occaſioning the electricity to paſs by fine points of communication on both ſides of the water; but, in fact, the reſemblance is not complete; for, in every way in which I have tried it, I obſerved that each wire gave both oxygen and hydrogen gas, inſtead of their being formed ſeparately, as by the electric pile.

I am inclined to attribute the difference in this reſpect to the greater intenſity with which it is neceſſary to employ common electricity; for, that poſitive and negative electricity, ſo excited, have each the ſame chemical power as they are obſerved to have in the electric pile, may be aſcertained by other means.

In the precipitation of copper by ſilver, an inſtance of deoxidation (or phlogiſtication) by negative electricity has been mentioned: the oxidating power of poſitive electricity may be alſo proved by its effect on vegetable blue colours.

Exper. 8. Having coloured a card with a ſtrong infuſion of litmus, I paſſed a current of electric ſparks along it, by means of two fine gold points, touching it at the diſtance of an inch from each other. The effect, as in other caſes, depending on the ſmallneſs of the quantity of water, was moſt diſcernible when the card was nearly dry. In this ſtate, a very few turns of the machine were ſufficient to occaſion a redneſs at the poſitive wire, very manifeſt to the naked eye. The negative wire, being afterwards placed on the ſame ſpot, ſoon reſtored it to its original blue colour.

By Mr. Volta's apparatus, the ſame effects are produced in much leſs time.

Beſide the ſimilarity which has thus been traced between the effects of electricity excited by the common machine and thoſe obſerved from the electric pile, I think it appears alſo probable that they originate from the ſame ſource.

With regard to the latter, its power is now known to depend on oxidation; ſo alſo does the excitement in the former appear very much to depend on the ſame proceſs; for,

Exper. 9. I have found, that by uſing an amalgam of ſilver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary, affords a good degree of excitement. Zinc acts ſtill better; but the beſt amalgam is made with both tin and zinc, a mixture which is more eaſily oxidated than either metal ſeparately.

Exper. 10. But, as a further trial whether oxidation aſſiſts in the production of electricity, I mounted a ſmall cylinder, with its cuſhion and conductor, in a veſſel ſo contrived that I could at pleaſure change the contained air.

After trying the degree of excitement in common air, I ſubſtituted carbonic gas, and found that the excitement was immediately deſtroyed, but that it returned upon readmiſſion of atmoſpheric air.

In conformity to this hypotheſis, we find that the metal oxidated is, in each caſe, in a ſimilar ſtate of electricity; for the cuſhion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and, in the ſame manner, zinc oxidated by the accumulated power of an electric pile, or ſimply by action of an acid, is alſo negative.

This ſimilarity in the means by which both electricity and galvaniſm appear to be excited, in addition to the reſemblance that has been traced between their effects, ſhows that they are both eſſentially the ſame, and confirms an opinion that has already been advanced by others, that all the differences diſcoverable in the effects of the latter, may be owing to its being leſs intenſe, but produced in much larger quantity.

XXXVII. *On the Preſenſation which Animals have of Changes in the Weather. By* Dr. F. A. A. MEYER, *at Göttingen* *.

I SHOULD only repeat what has been already long known, if I ſhould attempt to prove that, by the great enlargement of our knowledge reſpecting the natural hiſtory of foreign animals, by means of various learned travellers, we are now enabled to explain, to the ſatisfaction of the judicious naturaliſt, many phænomena which occur among indigenous animals. The ſenſations which take place in animal bodies before a change of weather, which animals expreſs by various external appearances, and which, without taking part in the diſpute whether animals are endowed or not with ſouls, may be called a *preſenſation*, ſeem however to require conſiderable explanation. I am well aware that it will be impoſſible for me to explain things which have not as yet been perfectly explained by men of the greateſt knowledge; but I am convinced that every thing which can contribute to the illuſtration of an obſcure ſubject deſerves to be ſubmitted to a proof, and that it is of uſe to communicate even ſingle obſervations on points concerning which ſyſtems cannot be formed till after the expiration of centuries. Conſidering the ſubject in this point of view, I preſent the following.

Preſenſation may be admitted under three heads:—I. The preſenſation which animals have of dry fair weather. II. The preſenſation which animals have of rainy weather. III. The preſenſation which animals have of ſtormy weather.

What regards the two firſt claſſes of the preſenſation of animals is taken from the Göttingen Pocket Almanac for the year 1779, the editor of which, as is well known, was at that period counſellor Lichtenberg. In that work I found the moſt authentic obſervations of the lateſt writers collected together. The obſervations reſpecting the third claſs I collected myſelf.

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* From *Magazin für das Neueſte aus der Phyſik*, vol. vii.

Firſt, then, reſpecting the preſenſation which animals have of fair dry weather.

Clear, dry weather generally follows after wet weather, when the atmoſphere has been freed from the vapours collected in it by their falling to the earth in rain. Clouds as well as rain are the means by which the air frees itſelf from the electric vapours that are continually ariſing; and if theſe again fall down, it appears very natural that animals, which live chiefly in the open air, ſhould expreſs, by various external movements, the eaſe with which they breathe, and perform all the vital functions. From this principle it ſeems not difficult to explain the following obſervations:

The fluttering of bats in the evening, beetles flying about on the highways, and the ſporting of gnats towards ſunſet, require no explanation. I ſhall only remark, what is already well known to every obſerver, that this preſenſation is highly uſeful to bats as well as to inſects. Every ſhower of rain would render it impoſſible for them to fly, as their wings are not ſecured by any oily matter againſt moiſture: they would therefore be rendered much heavier by rain, and unfit for flying, and they could not be ſo eaſily placed again in folds, which, conſidering the ſtructure of theſe animals, is abſolutely neceſſary, as when they have remained dry.

The ſame principle ſeems to be applicable to the high flight of larks and ſwallows, which perhaps haſten to the upper regions of the atmoſphere becauſe they are freer from vapours and more ſuited to them, and becauſe the lower regions, being more loaded with vapours, afford them leſs pleaſure than thoſe above. The inſects alſo which they purſue for food take then, perhaps, a higher flight.

The croaking of the green water-frog in ponds I cannot ſufficiently explain; but it ſeems to expreſs the pleaſure ariſing from the greater quantity of inſects then flying about, and which they can catch with more eaſe and convenience. But clear dry weather is not ſo agreeable to frogs as the return of warm weather. If they make a noiſe in the time of cold rain, warm dry weather will follow. But if the dry weather proceeds from raw winds, and if warmth and rain ſucceed, their noiſe may foretel rain; and therefore Linneus's rule *prædicit pluviam* will loſe nothing of its truth. He ſeems ſo much the more to be right, as more raw than warm dry days take place in the climate of Sweden. I have, to my great inconvenience, experienced the truth of his aſſertion, on journeys which I was under the neceſſity of continuing for ſeveral days.

That the weather-fiſh* (*cobitis foſſilis*) leaves the water quite pure during dry weather, and the green frog† ſits at the top of the glaſs, may proceed from the lighter or heavier ſtate of the atmoſphere, particularly as the latter is remarkably fond of cleanlineſs and moderately pure air.

The aſſembling of ravens in the fields, and the ſinging of the wood-pigeon, may be eaſily accounted for from the above principles.

I have never ſeen birds in good weather dreſs their feathers with oil from their fat glands, in order to ſecure them from rain; but I have obſerved many do ſo when the atmoſphere was overcaſt, and when there was an appearance of rain. I ſhould therefore include this circumſtance in the following claſs, did not experience admit alſo of another explanation, viz. that the birds, from the atmoſphere becoming lighter, hope for the ſpeedy arrival of dry weather, and therefore anoint themſelves, and ſecure their feathers from moiſture, that they may be able to fly higher than uſual with the leſs impediment. If the laſt explanation ought not to be altogether rejected, as I do not think it can, we may admit of this obſervation; eſpecially as all the experience of men worthy of belief allows of no reaſoning to be brought againſt it.

The expreſſion of animals which ſhow a preſenſation of rainy weather may be explained partly from the increaſing weight of the atmoſphere, partly from their manner of living, and partly from the want of moiſture which is neceſſary to their exiſtence.

The reſtleſsneſs of domeſtic cattle may proceed from many cauſes. It is known that the atmoſphere in ſummer, before rain falls, is generally heavier, on account of the electric vapour that ariſes. The inſects which infeſt cattle, and which mark this heavineſs, become then more numerous, and, getting into the ſtalls where the cattle are kept, torment them and make them reſtleſs. The aſcending vapour has alſo perhaps ſome influence on the ſkins of theſe animals, which ceaſes when the earth does not ſuffer ſo much vapour to eſcape as before; or the air, too ſtrongly charged with electricity, excites in them an unpleaſant ſenſation. It indeed appears ſtrange to explain the ſame phænomenon from two perfectly oppoſite cauſes, a want and an exceſs of electricity; but we know caſes of the like kind in medicine, ſuch, for example, as that where the cramp and ſleep produces atonia. People who have wounds or old ulcers feel, on a change of weather, a contraction and burning in thoſe parts; and why ſhould not ſuch affections take place in animals?

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* So called becauſe kept in Germany to foretel changes of the weather. When the weather is fine they continue quiet, but before a ſtorm or rain are very reſtleſs.—EDIT.

† This animal, though very common in many parts of Europe, is not found in England.—EDIT.

All theſe grounds taken together will be ſufficient, in my opinion, to explain why horſes and aſſes rub themſelves, ſhake their heads, and ſnuff the air by turning up their noſes; why aſſes bray much, and jump about; why cattle ſcrape up the earth, and ſtamp with their feet; and why ſwine, though not hungry, eat greedily and dig up the earth a great deal with their ſnouts. The reſtleſſneſs, running about, ſcraping with the feet, and eating graſs, among dogs; and moles continually throwing up the earth, can all be deduced from the ſame; as well as the cats dreſſing themſelves.

I have remarked that cocks crow on every change of weather, beſides at the uſual time. They as well as pigeons haſten to their places of ſhelter in order to be ſecured againſt the rain, the approach of which they muſt be ſenſible of, by the continually increaſing weight of the atmoſphere.

The cauſe of fowls, pigeons, quails, and other birds, waſhing themſelves, appears to me to be a certain heat or itching, which they wiſh by theſe means to remove.

Swallows, in all probability, take a low flight on the approach of rainy weather, becauſe the electric atmoſphere is too heavy for them, and becauſe they have not ſufficient ſtrength to mount above it. But cranes, as being ſtronger birds, employ all their ſtrength to riſe above it, and therefore fly ſo high.

I have remarked in ravens, that their croaking, unleſs when they ſmell carrion, proceeds from fear. They obſerve perhaps, by the atmoſphere ſtill becoming heavier, that a ſtorm highly diſagreeable to them will ſoon take place, and therefore they croak, and attach themſelves to trees; and when they are ſtartled by any thing uncommon, they take a high flight, making a loud cry. They eaſily diſcover their perſecutors among men, and always cry with a loud noiſe as long as they think they are purſued by them.

That jackdaws, on the approach of rainy weather, flap their wings, and pick their feathers with their bills, may be explained partly by an unpleaſant ſenſation before rain, and partly from the ſtate of the atmoſphere.

To the before-mentioned itching or barning ſenſation I refer alſo the bathing and plunging of water-fowl. That the birds of the foreſt ſhould haſten to their neſts is very natural, as from the ſtate of the atmoſphere they muſt apprehend rain.

The crying of peacocks, except at pairing time, appears to be a phænomenon analogous to the crowing of cocks. I have often remarked it on a change of weather, and often even on a change of wind.

That ſtorks and cranes place their bills under their wings, is a phænomenon remarked alſo among domeſtic fowls when they fly to their rooſts to ſecure themſelves againſt rain. Their pecking their breaſts ſeems to ſignify an itching ſenſation in that part of their bodies.

The croaking of the male green or tree frog ſeems to denote an unpleaſant ſenſation, for in fine weather I never heard them ſend forth the ſmalleſt cry. But the appearance of toads implies a pleaſant ſenſation, as theſe animals are ſo fond of living in dirt.

Ants labour with great diligence, and bees haſten home, and do not fly far from their hives, becauſe they follow their inſtinct. The former endeavour to complete their habitations and to ſecure themſelves better againſt rain, and perhaps to lay up proviſion for the rainy ſeaſon. The latter haſten home to their hives, and fly no more abroad, becauſe the wet would impede them in their flight and labour.

Gnats (*conops*) come into houſes to ſecure themſelves from rain, which would impede their flight, and there they attach themſelves to the legs to procure that nouriſhment which is denied them without.

The increaſed biting of fleas I cannot explain, as the natural hiſtory of theſe inſects is as yet too obſcure.

Earth-worms creep from their holes through inſtinct, as they can move themſelves forward only upon earth that is ſlippery.

A preſenſation of ſtorms I have obſerved only among the perfect of the *mammalia*, and as yet but among two, viz. man and the dog. Both theſe ſeem to have a ſenſe of the increaſed electricity of the atmoſphere. It appears in general, that the more imperfect animals remark only the approach of dry weather; the more perfect, the approach of rain; and the moſt perfect, the approach of ſtorms. All animals, perhaps, with their external ſenſes, and all plants by their organs, are ſenſible of the variations of the weather: but plants are not here my object, and it is not neceſſary to prove the influence of the weather on them, as it is ſufficiently apparent to every obſerver. Here I allude only to the external expreſſion of internal ſenſations, as may be ſeen by the adduced inſtances; elſe one might conſider the ſhutting and expanding of many plants as foreboding variations of the weather.

The dog, on the approach of rainy weather, expreſſes ſigns of uneaſineſs; ſcratches himſelf, becauſe the fleas then bite him with more violence; digs up the earth with his feet, runs round, and eats graſs: he is accuſtomed, however, to do the latter when he is very hot, perhaps to cool himſelf, and in general a ſtorm follows ſoon after. Before a ſtorm he evaporates more ſtrongly, ſo that his ſmell becomes intolerable; he creeps in a dejected manner to his maſter, and lies quiet. The cat alſo ſeems to have this in common with the dog, that ſhe creeps to her maſter alſo on the approach of a ſtorm. But all theſe phænomena require a further explanation.

The moſt perfect of all animals, man, is on the approach of ſtorms only ſubject to certain unpleaſant ſenſations; but theſe muſt teach him, in the moſt ſtriking manner, that his ſpiritual part, even though it diſengages itſelf ſo much from oppreſſive cares, is irremediably connected here below with a ſluggiſh body, which frequently exerciſes tyrannic ſway over the ſoul. Men in a ſound ſtate of health are ſubjected, on the approach of ſtormy weather, to a heavineſs of body and mind, a want of capacity to perform their uſual occupations, a yawning and relaxation, which are highly diſagreeable. Theſe are often accompanied alſo with a ſenſation of heat. All theſe phænomena appear in ſome more and in others leſs, and in ſome do not take place at all: but the laſt caſe happens very rarely. Sick perſons, or thoſe whoſe juices are corrupted, experience, beſides the above, an itching heat in thoſe parts of their bodies which are covered; and many who have old wounds, ulcers, and the like, have in theſe uncommon ſenſations. Many of theſe may be aſcribed to perſpiration checked by the great heat; though, as Weikard, a philoſophic phyſician, aſſerts, the want of electric matter in the body may have ſome ſhare in them alſo.

When ſtormy weather happens in winter, theſe ſenſations, as well as the before-mentioned preſenſation of animals, do not take place; at leaſt no one has ever obſerved them. This, in all probability, ariſes from the influence of the ſeaſon.

I muſt conclude this eſſay with requeſting, that what I have here ſaid may be conſidered, as it really is, an hypothetic explanation of well known facts.

XXXVIII. *Account of a Journey from the Fortreſs of Severnaja, at the northern Foot of Mount Caucaſus, to Choy in Perſia; with a Deſcription of Teflis, the Capital of Georgia.*

By P. F. ENGELMAN *.

WE ſet out on the 14th of January, and croſſed the Terek at Moſdok, accompanied by the Perſian ambaſſador † with his attendants, and having in our ſuite two officers and ſix Coſſacs of the Caucaſian legion, and an interpreter for the Perſian, Turkiſh, and Georgian or Gruſinian language. The caravan conſiſted of twenty-three horſes for carrying our baggage, and twenty-ſeven riding-horſes. The whole company were on horſeback, becauſe it is not poſſible to travel through the mountains in any other manner. By way of eſcort through the narrow defiles to the borders of Georgia, we were furniſhed with a guard of fifty regular Coſſacs and ſixty chaſſeurs.

On the 15th we croſſed the Leſſer Kabarda, which is ruled by ſeveral princes ſubject to the Ruſſian government, and next day arrived at Wladi-kaukas, a fortreſs lately conſtructed at the bottom of the higher Caucaſian ridge, in order to protect our communication with Georgia; from which we proceeded on the 17th to aſcend the mountains.

Caucaſus, which the Perſians, Indians, and Arabians call *Kukas*, extends from the Black to the Caſpian ſea. This ridge of mountains we had to croſs in its whole breadth to the capital of Georgia, from which the chain proceeds to Anatolia and along the Euphrates. Its greateſt length is from the mouth of the river Kuba to Derbent. A chain of ſnow mountains runs along the middle of this ridge from the Black to the Caſpian ſea. Two rivers flow from theſe ſnow mountains in this neighbourhood; one of which is the well-known Terek, that throws itſelf into the Caſpian ſea below the Ruſſian fortreſs of Kiſlar; the other, called Aragui, runs towards the ſouth, and falls into the Kur near the town of Zcheta, at the diſtance of about fifteen werſts from Teflis. Along the Kur and the Terek a commodious road was conſtructed, about two years ago, in order to open a free communication with Georgia; and this route in the ſummer-time is paſſable for carriages; but in the winter it is not poſſible to travel in that manner through the ſnow mountains, as the road between the mountains is covered with ſnow, and the travellers are obliged to paſs over ridges and eminences which are practicable only for perſons on horſeback or on foot. Along this road the high rocky mountains riſe in pyramidal forms, and like two walls of rock on each ſide; the valley, which forms a paſſage between them, where broadeſt may be about 300 yards. The people who inhabit the country in the neighbourhood of this road, in the mountains, and ſometimes on the river Terek, where the ſituation of the mountains will admit of it, are called Oſſi or Oſſetines. The former name was known in antient times, and is ſtill uſed among the people themſelves, but by the Ruſſians they are called Oſſetinzy. Their language has no affinity to any of the Aſiatic dialects but the Perſian. Thoſe who live to the north are ſtill pagans, and worſhip dogs, cats, and various other animals which have been ſtruck by lightning. Their habitations are all like fortreſſes, and ſurrounded by antique round towers, &c.

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* From *Neue Nordiſche Beyträge*, by profeſſor Pallas, vol. vi.

† The author accompanied, as conductor of the caravan, prince Tamara, who was ſent to Schiras and Iſpahan on a political miſſion, but who, on account of ſome circumſtances which took place, did not proceed thither.

On the 18th we arrived at the ſnow mountains, after being attacked at a narrow paſs on our way by the above-mentioned Oſſetines. The pretended cauſe was, that they demanded toll from us; but, as it had been ſettled by all the princes reſiding among the mountains, that every thing belonging to Ruſſia ſhould be ſuffered to paſs free, our commander would not comply with their demand, nor pay them a ſingle farthing. When the Oſſetines found that they could obtain nothing, they began to roll down on us aſtoniſhing large maſſes of rock: and they diſcharged alſo their fire-arms, but without doing us any hurt. Our ambaſſador, however, was ſo incenſed, that he commanded 60 of the chaſſeurs and 150 of the Coſſacs, who formed our eſcort, to march againſt them; but they could not reach them but by a circuitous route of about forty werſts. When the Oſſetines ſaw that this party were going round to attack them in the rear, they ſolicited forgiveneſs, and ſuffered us to paſs unmoleſted.

On the 19th we continued to aſcend the ſnow mountains, where we were much aſtoniſhed at the ſudden change of the climate. Hitherto the weather had been temperate, and we had enjoyed the moſt beautiful views, highly picturesque on account of the different colours of the rocks, which in many places were ornamented with a ſort of cryſtallized figures. In the month of March, Caucaſus muſt be a real paradiſe; for the cypreſs-like juniper trees, the cheſtnut, and other beautiful trees, are every where found mixed in the moſt agreeable manner. Travelling, however, through the ſnow mountains was not ſo pleaſant. Where we croſſed them they conſiſted of about five ridges running parallel to each other, the middlemoſt or higheſt of which roſe above the clouds. Each ridge is from a half to three quarters of a German mile in breadth. When we reached the ſummit of the firſt mountain we experienced a moſt violent cold, and the ſnow lay on the ground above an arſchine and a half in depth. As this mountain was not very ſteep, we paſſed it without much difficulty. When we reached the bottom we had to croſs the river Terek; but no bridge was neceſſary, for the river is continually frozen, and the ice is covered by ſnow accumulated for many years, and which never diſſolves in ſummer however hot the weather may be; and if it were not for the prodigious noiſe which the water makes, no one would ſuſpect that ſo rapid a ſtream flows under it.

We then paſſed the ſecond ridge, which however ſeemed to be far more dangerous, as it is remarkably ſteep. When we had aſcended half way the road became ſo narrow, that between the perpendicular rock on the one ſide, and the precipice, more than 400 fathoms in depth, at the bottom of which is the bed of the river, only two or three feet were left for us to paſs. While in this ſituation a violent wind aroſe, as is commonly the caſe, and drove down a prodigious quantity of ſnow, which, as misfortune would have it, fell upon our ambaſſador, his ſteward, and a riding boy, and hurried them all together, with their horſes, to the bottom. Luckily there happened to be in this place a projection of rock, on which they all three fell; but ſo covered with ſnow that for ſome minutes they could not be ſeen, until they had forced their way from the ſnow with their heads. We immediately let down to them, by means of ropes, a few Coſſacs and Oſſetines, who are very expert on ſuch occaſions. Theſe people, having deſcended in this manner at leaſt 140 fathoms, helped the ambaſſador and the other two perſons from the ſnow, and, having made them faſt to ropes, they were drawn up without any farther accident. Amidſt fear and terror we proceeded with the caravan through this dangerous place, dreading every moment that a new fall of ſnow rolling down from the mountain would hurry us to the bottom of the precipice. By the protection of Providence, however, we were preſerved, and arrived in ſafety at the top of the mountain. Our ambaſſador was not hurt by his fall, but greatly frightened. Two of the horſes, contrary to all expectation, found means to clamber up the ſteep ſide of the mountain; and though they often fell among the ſnow ſo that nothing of them was to be ſeen except the head, they made their way through it; but the third was too weak; and while exerting the laſt remains of his ſtrength by a ſudden ſpring, he fell back and rolled down the precipice, ſo that we ſaw no more of him.

We now deſcended the mountain, the road being neither very ſteep nor long, till we arrived at the next ridge, which is, as it were, placed upon the former. This third and higheſt ſnow mountain was ſo ſteep that we almoſt deſpaired of being able to paſs it. We found it now impoſſible to ride; for, though the road was here and there two feet in breadth, we were frequently obliged, for the diſtance of half a werſt and more, to clamber over the rocks ſometimes up to the middle in ſnow. When we had at length, with great labour, pain, and fatigue, got to the ſummit, we obſerved the clouds rolling under us, and could no longer ſee any thing of the ſurrounding country. We now travelled above an hour, involved in a ſort of thick fog, expoſed to the wind and ſnow, and to a more penetrating cold than we ever remembered to have experienced at Peterſburgh.

When we began to deſcend from this high ridge, and had got about half way down, three of the mules laden with the ſervice of ſilver plate deſtined as a preſent to Perſia, fell into a ſmall fiſſure near the road, which at firſt, as we ſaw their heads above the ſnow, did not appear to be of much conſequence. Theſe animals, however, exerted their whole ſtrength to extricate themſelves, and two of them were fortunate enough to ſucceed; but the third, which had approached too near to the declivity of the mountain, by its exertions fell deeper into the ſnow, and, getting ſtill nearer and nearer to the edge of the precipice, rolled down the ſide of the mountain to the depth of about 400 fathoms from the road. The boxes containing the plate broke looſe by the violence of the fall, one rolling down on one ſide and the other on the other; and one of them, being daſhed againſt the projecting point of a rock, burſt in two; but the other fell unhurt on a ſmall eminence which was at the diſtance of two hundred fathoms from us. In that ſituation we were obliged to leave it, as it was impoſſible to approach it on account of the ſteepneſs of the mountain. A Gruſinian (Georgian) prince, whom the czar ſent to meet us on the frontiers, promiſed, as ſoon as the weather would permit, and the new ſnow ſhould be melted, to take proper care that the Oſſetines in the next village ſhould bring up the box which remained whole, and alſo collect the ſcattered plate which had fallen from the box that had burſt, and which could not ſink into the old hardened ſnow, as it is ſo ſtrong even in ſummer as to bear the weight of a horſe. On our return, indeed, one of the boxes with the plate was brought to us at Teflis, and all the plate which had fallen from the broken one was again found, except ſix gilt ſilver diſhes, with one large and two dozen of ſmall ſilver ſpoons; of the gold deſſert ſervice, ſeven knives and ſome ſmall diſhes were recovered; and, of the gilt, thirteen knives and ſome forks *.

Having again proceeded on our journey, we at length, about one o'clock in the morning, after great pain and difficulty, on account of the ſnow, which was continually rolling down from the mountain, arrived without further accident at a village at the bottom of the ridge diſtant about eighteen werſts, which had coſt us eighteen hours labour from the village where we had ſpent the preceding night. This village was one of the pooreſt and moſt wretched in all Georgia; but to us it was a welcome place of refreſhment, where we remained during our twentieth reſting day, and, after three days journey more, arrived in ſafety on the 23d of January at Teflis, the capital of Georgia. This place we reached at midnight; and though it was ſo dark that we could not ſee a yard before us, we concluded from the dirt and narrowneſs of the ſtreets, and from the diſagreeable effluvia which aſſailed us from every corner, that, when day-light appeared, we ſhould find ourſelves neither in a Berlin nor a Dreſden: and this indeed was the caſe, as will appear from the following deſcription.

But we ſhall here firſt give a ſhort view of the province of Tuoletti, which we traverſed in our way from the ſnow mountains to Teflis. At about the diſtance of twenty-five werſts from the ſnow mountains the country begins to be exceedingly pleaſant, as the mountains gradually become lower, and are covered with wood to the tops. The valley along the river, which forms the principal paſſage from Ruſſia to Georgia, ſtill grows wider the nearer it approaches the capital; ſo that, where the Aragui diſcharges itſelf into the Kur, which runs through Teflis, about twenty werſts from that city, the breadth of the valley is from five to ſeven werſts. In other places its breadth varies from a half to a whole werſt, and is every where interſperſed with the moſt beautiful groves of beeches, cheſtnut, apple, and pear-trees. Between theſe fruit-trees vines ſhoot up, and the bruſh-wood is of that kind generally ſeen in Europe. Through this woody vale the Aragui flows in different branches, and the route in winter and the end of ſummer goes through the woods. No proſpect more beautiful can be conceived than

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* As theſe articles were deſtined as a preſent to an eaſtern prince who ate rice with his hand, it does not appear that they would have been of much uſe to him.

that of the mountains on each ſide of this valley. At about the half of their height lie in general the villages, which, according to the Aſiatic mode, are built with flat roofs. Where any of the princes reſide, the village is ſurrounded with a high wall, ſtrengthened by old round turrets. In this manner, two or three villages often lie one above the other, each having its gardens and fields laid out on the declivity of the mountain; ſo that it appears wonderful how it is poſſible to cultivate them. Rivulets every where fall, in the form of caſcades, from the rocky brows of the mountain; and this diſtrict, in an amphitheatrical form, extends above ſixty werſts* with the moſt agreeable variety; ſo that a more beautiful landſcape can hardly be found either in Swiſſerland or Italy.

When compared with this beautiful neighbourhood, Teflis, the capital, makes a very poor appearance. It is about eight werſts in circumference, and is divided into two parts by the river Kur, which flows through it. The principal part of the town lies on the declivity of a high mountain, which terminates at the river with rocky banks of from eight to eleven fathoms in height. On the land ſide it is incloſed by a wall, built in the old form, eight fathoms in height, and at the diſtance of every fifteen fathoms ſtands a large round tower which ſupplies the place of a baſtion. On this ſide, the city has three gates; and towards the mountain, which riſes almoſt perpendicularly behind the wall, it is protected by an old caſtle, which is exceedingly ſtrong, and which exhibits the true old method of building. This caſtle is ſituated at the elevation of at leaſt two hundred fathoms above the level of the city. The other part of the city is connected with the former by means of a wooden bridge, which is only twenty-five fathoms in length; as the river Kur, though in other places from a hundred to two hundred and eighty fathoms in breadth, is here confined on both ſides by high rocky banks, ſo that it never can become broader; and its real breadth under the bridge amounts only to twenty fathoms. This part of the city is fortified in the ſame manner as the former, and contains the czar's palace, a church, and a few houſes belonging to the citizens. In the whole city there are ſeventeen churches, Georgian and Armenian, with one catholic church and three moſques. The number of the houſes in proportion to the extent of the ground is very great, as the breadth of each houſe in front is only from four to twelve fathoms. They are all irregularly built; ſome of them have neither windows nor chimneys, as the flat roof has in the middle a ſort of cupola with a round aperture in it, which ſerves both to admit light and to afford a paſſage to the ſmoke. The fire is made in the middle of the floor, and round it are the ſleeping places: people even of the middle claſs reſide in houſes of this kind.

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* A werſt is about 4200 Engliſh feet.

The city is interſected by an aſtoniſhing number of ſtreets, ſhort and crooked, and not above two fathoms in breadth; one principal ſtreet only which is capable of admitting a carriage paſſes through the whole city. In my opinion there is not a dirtier place in the world than Teflis; for when the ſnow falls in winter it lies ſometimes on the flat roofs of the houſes to the depth of two or three feet, and is thrown down into the ſtreets by the inhabitants by means of ſhovels, leſt it ſhould penetrate into the houſes by its melting, which generally takes place very ſoon. As the ſtreets are not paved, this ſnow occaſions ſo much dirt, that to perſons on foot they are almoſt impaſſable. Beſides, carrion of every kind, ſuch as dead cattle, ſwine, dogs, cats, and the like, is thrown out into the ſtreets, where it is ſuffered to become putrid. Even cloſe to the ezar's palace I obſerved the remains of a dead cow, which emitted an intolerable ſtench, and would have long continued a nuiſance had it not been devoured by the dogs. In winter the ſtench is not ſo offenſive as in ſummer during the great heats, when ſtrangers are almoſt ſuffocated by it; and on this account it is very ſurpriſing that peſtilential diſeaſes are not as prevalent in Teflis as at Conſtantinople. Strangers immediately on their arrival are ſenſible of the impurity of the air, and in conſequence of it are continually ſick. The healthieſt time here is the autumn, becauſe the ſtrong winds which blow then from the mountains purify the air; but they would produce a much better effect if the city were regularly built, for at preſent the ſtreets are too crooked and narrow to afford it a free paſſage.

The ezar's palace ſtands on the rocky bank of the river; the lower part of it is built of ſtone, the upper is of wood and ſurrounded by a gallery; but in other reſpects it has a very mean appearance. In the interior part it exhibits nothing remarkable; all its ornaments conſiſt merely of rich and beautiful carpeting. The area before it is often ſo covered with dirt that it is impoſſible to approach the palace. The magnificence of the court is, in my opinion, much inferior to that which we obſerved at the court of the Perſian chan Achmet Mehemet, at Eriwan; who, however, is ſubject and tributary to the ezar of Georgia. To deſcribe the etiquette of the court would be too tedious and ridiculous; I ſhall therefore only obſerve, that prince Heraclius is a prudent, intelligent, and courageous man, whoſe talents are diſplayed in his phyſiognomy. He is of a middle ſtature, and ſtill freſh and vigorous though ſeventy years old. He has a great many children, the eldeſt of whom is forty-five years of age. The czar is exceedingly polite, and on days of feſtivity invites all the Ruſſian officers to his table. I had twice this honour. We were ſerved in the European manner; the diſhes however, for the moſt part, were cooked according to the Aſiatic taſte, but excellent of their kind. On ſuch occaſions the czar ſits at table as the Europeans do, but in common he follows the Aſiatic cuſtom.

The czar's conſort and the princeſſes often appear publicly on feſtivals; but ſome years ago they were more reſerved, being ſeldom ſeen. The children are all ſtout and handſome.

Of the Tartars ſubject to the czar of Georgia, and who are diſtinguiſhed by the name of *Kaſach*, I obtained at Teflis the following information:—They reſide on the rivers Nachatyr, Tebete, Kur, Achyſtaf, Pambek, and Algete. On the firſt lie ſeven villages; on the ſecond, thirteen; on the third, five; on the fourth, fifteen; on the fifth, eighteen; and, on the ſixth, two. Some of theſe Tartars are of the ſect of Omar; others are of that of Ali, and do not practiſe circumciſion. They acknowledge, beſides the Supreme Being, five ſaints. David, who had a hundred wives, and therefore Mahomet allowed his followers to have the ſame number; Dſchebrail, who, though inviſible, excites the people to offer up their prayers to God; Aſrail, who by God's command ſets the ſoul free from the body; Michael, who weighs the ſins of the dead, and ſends them either to hell or to paradiſe; and, in the laſt place, Iſraſil, who, when God ſhall make the water like the earth, will ſound the trumpet to call the dead to judgment. Mahomet they do not account a ſaint, but their lawgiver, who has commanded them to pray five times in the twenty-four hours, and to implore God to forgive their ſins; to purify themſelves before prayer by waſhing; to undertake a pilgrimage to his grave when poſſible; not to ſteal; not to work on Friday, but to faſt; to obſerve the feſtivals, &c.

They divide the year into twelve months, and aſſign to the months an unequal number of days, which vary every year except for the month Ramaſan, which always conſiſts of thirty, and in which their principal feſtivals fall. Their week has ſeven days, and begins with Friday, which they call Dſchuta: the reſt are, Dſchuta-Erteſſi, Baſar, Baſar-Erteſſi, Chas, Tſcharſchembe, Perſchembe. They have three grand feſtivals. The firſt is in the month Ramaſan; the ſecond is called Kurban, and the third Naurus-Bairam.

Their great faſts in Ramaſan are obſerved ſo ſtrictly, that they ſpend whole days in prayer, and taſte no food till the evening.

At their marriages no other ceremonies are uſual than, at the time of the betrothing, the male and female friends aſſemble at the houſe of the bride, and ſing from morning till night, dance, and make merry, till the wedding day; after which they ſpend three more days in a ſimilar manner. At the birth of a child, the midwife, after it has been waſhed, beſprinkles it with ſalt water, and gives it any name ſhe thinks proper. A woman, after delivery, is accounted unclean for forty days; during which ſhe muſt neither ſleep with her huſband, pray in the church, nor perform any domeſtic office. Marriage is forbidden only between the firſt degrees of conſanguinity, ſuch as that of brother and ſiſter; in all other degrees it is permitted. A man may keep as many wives as he chooſes: the firſt muſt always be a maid, but the reſt may be widows. If a married woman is guilty of adultery, the huſband has a right to put her to death, as well as the adulterer. If a huſband, however, is unfaithful, the wife has no reſource. Men are accounted fit for marriage from the age of fifteen to fifty, and women from that of twelve to forty. If a man loſes all affection for his wife, he may give her a letter of divorce and ſend her away; but a woman cannot ſeparate from her huſband without his conſent.

They bury their dead in the following manner:—The body is waſhed, and a *mullah*, or prieſt, is ſent for to accompany it to the grave; the mullah then recites ſome prayers, and the body being depoſited in the grave the relations cover it with earth. The relations do not return to the houſe of the deceaſed for three days, but at the expiration of that period they prepare an entertainment in honour of his memory; and this feſtival is repeated on the ſeventh, and again on the fortieth day.

During nine months of the year theſe people remain ſtationary in their villages; but after the firſt of July they retire with their cattle to the river Tebete among the mountains, where they lead a wandering life till the firſt of October. To the fleſh of deer, ſwine, and horſes, they have an averſion. Their language is a ſomewhat corrupted dialect of the Tartaric.

A kind of borax, brought from Akalzi and Wan, is ſold at Teflis, for the uſe of the goldſmiths, under the name of *bora*[*]. At Oldi, near the former of theſe places, about three days journey from Teflis, and at Bajaſid, near the latter, there are ſaid to be ſprings, the water of which is greeniſh.

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[*] This is the impure mineral lixivious ſalt deſcribed by Model under the name of *ſal Perſicum.*

It is collected in ſtone ciſterns, where it is ſuffered to remain three or four months with ſhavings of leather, blood, and urine, till it becomes putrid; after which it is clarified, and boiled in copper kettles. A concrete ſalt remains, which is the *bora*.

After a reſidence of almoſt three weeks at Teflis, during which time we put our equipage again into order, and purchaſed freſh cattle, we ſet out on the 12th of February, the weather being then as mild as it is at Peterſburgh in the ſpring. In general, ice is ſeldom ſeen in the neighbourhood of Teflis; and the weather in winter is, for the moſt part, rainy. Some opinion reſpecting the climate of this country may be formed from the fruit-trees, ſuch as the pomegranate- and fig-tree, which grow here in the open air in all the gardens, and yet never ſuffer from the froſt.

We had to repaſs the mountains, but the road now was not ſo dangerous. On the 17th we arrived at the bottom of that height called the Bambakſche ridge. During theſe five days we had an agreeable journey through a beautiful diſtrict every where interſected by ſmall ſtreams, and covered with fruit-trees of every kind. The inhabitants of this agreeable diſtrict are, however, much to be pitied, as they are continually expoſed to the depredations of the Leſghinians. In the courſe of one day's journey we ſaw thirteen villages which theſe plunderers had deſtroyed. To ſuch ſcenes of devaſtation the Georgians are often expoſed, as the Leſghinians reſide in the mountains between Kachet and Dageſtan, and are exceedingly numerous and warlike. The pacha of Achalziche every year takes from two to three thouſand of theſe people into his ſervice, but gives them little pay, as they receive permiſſion from him to indemnify themſelves in their march by what they can plunder. To ſtrengthen this corps he unites with them ſome Turkiſh troops. Some time ago, when three companies of our chaſſeurs, under the command of major Sengenberg, defeated the Leſghinians, of the two thouſand killed, one-half almoſt were Turks. On another occaſion, when four hundred remained on the field of battle, twenty Turks and two of their ſtandards were brought into Teflis. Since that period the Leſghinians have been afraid of our chaſſeurs; but the two battalions which we maintained in Georgia were not ſufficient to defend it from theſe robbers*.

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* Since that period Georgia has been annexed to the Ruſſian empire. The reaſons which produced this event will be found in the following extract of a letter from Peterſburgh, dated October 30, 1801:

"The manifeſto by which his Imperial Majeſty declares that Georgia, called

On the 18th our road lay over the cold ſnow mountains from Bamback. We were ſtill eighty werſts from the firſt Perſian town Eriwan, and had aſcended the ridge, which extends ſixty werſts; when all of a ſudden, after having travelled five werſts, we were aſſailed by a violent wind, which accumulated the ſnow in heaps before us; ſo that we were obliged to return, to avoid being buried alive: a cataſtrophe which whole caravans ſometimes experience among theſe mountains, as they muſt travel here ſixty werſts without finding any village or place of ſhelter, and conſequently can hope for no aſſiſtance when overtaken by a ſnow ſtorm.

On the 19th of February we purſued another route, in order to avoid, in ſome meaſure, the ſnow mountains. This route however was full 150 werſts longer, and exceedingly

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called by the Ruſſians Gruſinia, is united to the crown, gives an account of the motives that determined this meaſure. The ſovereigns of Ruſſia have always conſidered it as a duty to protect that ſtate: the withdrawing of the Ruſſian troops from it had occaſioned every kind of diſorder, and civil war had often burſt forth. The barbarous tribes by whom it is ſurrounded, threatened it on every ſide; and without the Ruſſians it was impoſſible to repreſs the banditti of Mount Caucaſus. When the emperor Alexander aſcended the throne, he found Georgia united to the crown, in conſequence of a manifeſto dated January 18, 1801. However, as the Ruſſians had produced tranquillity in it, the emperor was deſirous of trying to reſtore to it its independence; but this country had again to dread the vengeance of thoſe who had been excluded from the ſovereignty—it was threatened with a new civil war. The prince by whom it was governed could not ſecure it either internally or externally, and to all theſe circumſtances was added the wiſh of the Georgians themſelves. 'Your hopes this time,' ſaid the emperor to them, 'ſhall not be deceived. It is not to increaſe my power; it is not for intereſt; it is not to extend the boundaries of an empire already ſo extenſive, that we have taken upon us the government of Gruſinia: our dignity alone, our honour and humanity, impoſe on us the ſacred obligation of liſtening to the voice of thoſe who ſuffer; and to eſtabliſh in Gruſinia a juſt government, which may enſure the ſafety of perſons and property, and the protecting force of the law.'

"Theſe are the intentions of our auguſt monarch. The produce of the taxes of Georgia will not go out of the country; it will be conſecrated to the ſupport of government, to the reparation of towns and villages ruined by inteſtine war, and to the relief of the inhabitants. Each will be maintained in his rights, and in the exerciſe of his religion. The children of the princes will retain their eſtates, and may live where they pleaſe. Lieutenant-general Knorring, a native of Georgia, will be charged with the government; and in order that he may carry into effect the inſtructions of the emperor, he will aſſociate with him other perſons born in the country. All the Georgians will take an oath of fidelity to their new ſovereign, and the eccleſiaſtics and paſtors will be the firſt to give this example of ſubmiſſion.

"His majeſty concludes with expreſſing an ardent wiſh that peace and juſtice may prevail in the country; that an end may be put to acts of violence; and that agriculture, induſtry, commerce, and manufactures, may revive, under the protection of the new laws, equal to all."—Edit.

dangerous, as we were obliged to paſs very near the Turkiſh borders, which are generally infeſted by the Leſghinians.

On the 23d we arrived at the village of Talin, which belongs to Armenia the Leſs. Scarcely had we got to our lodging when intelligence was received that 2000 Leſghinians had croſſed the river behind us, and attacked a village which we paſſed a little time before. Had we fallen in with theſe people on the road, our ſilver plate muſt have been loſt.

On the 24th we came to the celebrated Armenian monaſtery of Weds, or Edſch-mijazin (ſeven churches), which is called alſo the Monaſtery of Ararat, where the Armenian patriarch, the chief of the eccleſiaſtical eſtabliſhment of that country, has his reſidence. Here we were received with great hoſpitality, lodged in an elegant apartment, and waited on with great attention. Next day I made a drawing of the monaſtery, together with Mount Ararat and the neighbouring country; which pleaſed the patriarch ſo much when ſhown to him by our commanding officer, that he requeſted me to make a copy of it for him, and to encourage me to do ſo he gave me a conſiderable preſent.

This monaſtery is ſaid to have been built upwards of 1300 years ago, in the reign of an Armenian king named Tartat: and beyond all doubt it is one of the richeſt in the world, as it receives a great income from the Armenians ſcattered throughout Turkey, Perſia, Georgia, and even Europe; above 800 peaſants alſo belong to it, and a large tract of land, on which 3‸0 head of cattle, 100 horſes, and 50 camels, are kept for the purpoſes of agriculture. When the patriarch dies, a new one is choſen from their ſynod; and an account of this choice is then tranſmitted to the Turkiſh ſultan, the czar of Georgia, and the chan of Iſpahan. The number of the monks amounts to 120, who are lodged in the monaſtery, where they enjoy every convenience. Travellers are ſhown here a fragment of Noah's ark, which a monk is ſaid to have obtained from an angel. This monk had reſolved to aſcend to the ſummit of Mount Ararat, where the ark reſted; but he was prevented by the Deity, who ſent an angel to convey him back; and the angel, to conſole him for this diſappointment, brought him this fragment, reſpecting which many other wonderful ſtories are told.

On the 26th we continued our journey to Eriwan, which is only fifteen werſts diſtant from the monaſtery. This city lies in a fruitful valley on the river Sangintſchey, at the bottom of the ſnow mountains. It is fortified by a double wall, furniſhed with a great many round towers at the diſtance of fifteen fathoms from each other, and each mounted with four cannon: mortars alſo are planted on the edge of the ditch by which the city is ſurrounded. The city has only one gate, and would be a place of ſtrength if its ſituation were more advantageous. One-half of it is ſurrounded by mountains within cannon ſhot; but on the ſouth ſide it is protected by inſuperable rocks, which extend, almoſt perpendicularly, to the banks of the river Sangin. The houſes are in the ſame ſtyle of architecture as thoſe of Teflis.

The chan, Achmet Mehemet, was about ten years of age, and ſucceeded his brother, who had been aſſaſſinated in the ſtreets about a year before. The diſtrict belonging to Eriwan forms a great part of Armenia the Leſs: it is exceedingly fertile, and brings to the chan a conſiderable income; from which, however, he muſt pay a tribute to the czar of Georgia.

On the 27th our colonel paid a viſit to the chan, all of us accompanying him in great parade on horſeback. When we arrived at the court of the palace we all diſmounted, the colonel excepted, who rode up to the palace, where he was received by one of the chan's vaſſals, who conducted him into the hall of audience, to which all the officers followed him. Chairs covered with red cloth were handed to us; after which we were preſented with coffee, tea, and ſweetmeats; and then with the *kallian*, a kind of machine uſed for ſmoking tobacco through water. The audience laſted about half an hour, during which the chan ſpoke very little, and of courſe we had no opportunity for much converſation. When the audience was ended, we returned to our lodgings in the chan's ſummer palace. The ſtreets through which we paſſed were ſo full of people, that it was with difficulty we could get along.

On the 1ſt of March we continued our journey towards Nachtſchiwan, the capital of Armenia; but when we arrived at Derwiſchlär, ſixty-nine werſts from Eriwan, we were obliged, on account of the danger of the road, to return to Eriwan, as the Curds along the river Aras were in a ſtate of inſurrection, and had already committed many acts of robbery. Theſe Curds are a people nearly in a ſavage ſtate, who lead a roving life among the mountains. Their habitations conſiſt of a kind of half tent made of a ſort of hair cloth, and which has a reſemblance to a piquet tent. Such tents are inhabited by whole families. They live chiefly on vegetables, and their dreſs is Turkiſh.

On the 4th we arrived again at Eriwan, where we ſtaid till the 18th, when we ſet out, under the eſcort of 150 men, for Nachtſchiwan, which we reached on the 20th of March.

XXXIX. *Account of the Proceſs employed in making Cheſhire Cheeſe*.*

Preparation of the Rennet.

WHEN the maw ſkin comes from the butcher, the chyly matter is taken out, and the ſkin cleaned from ſlime and every apparent impurity, by wiping or gently waſhing. The ſkin is then filled nearly full of ſalt, and, placing a layer of ſalt upon the bottom of a mug, the ſkin is laid flat upon it: this mug is large enough to hold three ſkins in a courſe: each courſe of ſkins ſhould be covered with ſalt; and when a ſufficient number of ſkins are thus placed in the mug, that mug ſhould be filled up with ſalt, and, with a diſh or ſlate over it, be put in a cool place, till the approach of the cheeſe-making ſeaſon the following year. The ſkins are then all taken out and laid for the brine to drain from them; and being ſpread upon a table, they are powdered on each ſide with fine ſalt, and rolled ſmooth with a paſte roller, which preſſes in the ſalt. After that, a thin ſplint of wood is ſtuck acroſs each of them to keep them extended while they are hung to dry. Take all the maw ſkins provided for the ſeaſon, pickled and dried as before; put them into an open veſſel or veſſels, and for each ſkin pour in three pints of pure ſpring water. Let them ſtand twenty-four hours, then take out the ſkins, put them into other veſſels; add for each one pint of ſpring water, let them ſtand for twenty-four hours as before. On taking the ſkins out the ſecond time, gently ſtroke them down with the hand into the infuſion. The ſkins are then done with. Mix thoſe two infuſions together, paſs the liquor through a fine linen ſieve, and add to the whole a quantity of ſalt, rather more than is ſufficient to ſaturate the water; that is to ſay, until a portion of the ſalt remains undiſſolved at the bottom of the veſſel. The next day, and alſo the ſummer through, the ſcum as it riſes is to be clearly taken off; and as the liquor ſhould not be ſuffered to remain without a portion of undiſſolved ſalt at the bottom, it will be neceſſary to add frequently freſh ſalt, as that which was diſſolved will gradually form itſelf into cryſtals, and be taken off with the riſing ſcum. Somewhat leſs than a wine half-pint of this preparation will be generally ſufficient for 60 lbs. of cheeſe. Whenever any of this liquid is taken out for uſe, the whole ſhould be well ſtirred up.

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* From the Agricultural Report of that County.

Colouring for the Cheſhire Cheeſe.

The colouring for cheeſe is, or at leaſt ſhould be, Spaniſh annotta; but as ſoon as colouring became general in this country, a colour of an adulterated kind was expoſed for ſale in almoſt every ſhop: the weight of a guinea and a half of real Spaniſh annotta is ſufficient for a cheeſe of 60 lbs. weight. If a conſiderable part of the cream of the night's milk be taken for butter, more colouring will be requiſite. The leaner the cheeſe is, the more colouring it requires. The manner of uſing annotta is, to tie up in a linen rag the quantity deemed ſufficient, and put it into half a pint of warm water over night. This infuſion is put into the tub of milk in the morning with the rennet infuſion; dipping the rag into the milk, and rubbing it againſt the palm of the hand as long as any colour comes out.

Setting the Cheeſe together.

It is, we believe, generally admitted, that not only the quantity, but the quality of the curd, as to texture, viz. toughneſs or otherwiſe, depends in a great meaſure upon the length of time the cheeſe is in coming; and that the time, again, depends on the quantity and ſtrength of the coagulum uſed, the ſtate of the atmoſphere, and the heat of the milk when put together. In this ſtage of the art, where a degree of accurate certainty ſeems to be required, there is no other guide but the hand and the external feelings: the thermometer of a Cheſhire dairy-woman is conſtantly at her fingers' ends: accordingly the heat of the milk when ſet, is endeavoured to be regulated by the ſuppoſed warmth of the room and the heat of the external air; having reference alſo to the quantity and ſtrength of the ſteep, ſo that the milk may be the proper length of time in ſufficiently coagulating, which is generally thought to be about an hour and a half. The evening's milk of ſuppoſe twenty cows having ſtood all the night in the cooler and braſs pans, the cheeſemaker in ſummer, about ſix o'clock in the morning, carefully ſkims the cream from the whole of it, obſerving firſt to take off all the froth and bubbles, which may amount to about a pint; this, not being thought proper to be put into the cheeſe, goes to the cream-mug to be churned for butter; and the reſt of the cream is put into a braſs pan. While the dairy-woman is thus employed, the ſervants are milking the cows, having previouſly lighted a fire under the furnace, which is half full of water. As ſoon as the night's milk is ſkimmed, it is all carried to the cheeſe-tub, except about three-fourths of a braſs pan full, viz. three or four gallons, which is immediately placed in the furnace of hot water in the pan, and is made ſcalding hot; then half of the milk thus heated in the pan is poured alſo into the cheeſe-tub, and the other half is poured to the cream, which, as before obſerved, is ſkimmed into another braſs pan. By this means all the cream is liquefied and diſſolved ſo as apparently to form one homogeneous or uniform fluid, and in that ſtate it is poured into the cheeſe-tub; but before this is done ſeveral bowls or veſſels full of new milk will generally have been poured into the cheeſe-tub, or perhaps the whole morning's milk. Care is taken to ſkim off all the air-bubbles which may have formed in pouring the new milk into the cheeſe-tub.

The rennet and colouring being put into the tub, the whole is well ſtirred together, a wooden cover is put over the tub, and over that is thrown a clean linen cloth. The uſual time of coming is an hour and a half, during which time it is to be frequently examined: if the cream riſes to the ſurface before the coming takes place, as it often does, the whole muſt be ſtirred together, ſo as to mix again the milk and cream, and this as often as it riſes, until the coagulation commences. A few ſmart ſtrokes on different ſides of the tub with the cheeſe-ladder, &c. will forward the coagulation, if it is found too long in forming. If the dairy-woman ſuppoſes the milk, &c. to be accidentally put together cooler than ſhe intended, or that its coolneſs is the cauſe of its not coming, hot water or hot milk may be poured into it, or hot water in a braſs pan may be partially immerged therein: but this muſt be done before it is at all coagulated; for, after that takes place, though but imperfectly, it muſt not be tampered with ſo as to break the forming curd, for a conſiderable part of the cream would thereby be directed into the whey, and the quantity of curd much leſſened. Before the coagulation takes place, an additional quantity of rennet may alſo be put in, if thought neceſſary; but this will, after coagulation, be added with little effect, as no means can be uſed to mix it with the whole maſs without diſturbing the forming curd. If the cheeſe has been ſet together hotter than it was meant to be, the oppoſite means, under the ſame precautions, may be recurred to. But the more general practice is, to ſuffer the proceſs to proceed hot as it is, until the firſt quantity of whey is taken off; a part of which, being ſet to cool, is returned into the tub to cool the curd. When the cheeſe happens to come much ſooner than a proper time, owing to too great a degree of heat in the milk in ſetting or putting together, or too great a ſtrength of ſteep, there is leſs curd, and it is conſiderably

ſiderably tougher than when the milk has been ſet cooler together than uſual, or when too little ſteep has been uſed. In the latter caſe, the curd is exceedingly tender: and, when that ſo happens, a part of the whey is taken out of the cheeſe-tub, and heated as much as may be thought ſufficient to give to the curd, when mixed with it, a proper degree of toughneſs. In an hour and a half, as mentioned before, if all goes on well, the coagulation will be formed. This point is determined by gently preſſing the ſurface of the milk with the back of the hand.

Breaking down the Curd, Gathering, &c.

If the milk has been ſet together very warm, the curd, as before obſerved, will be firm: in this caſe, the uſual mode is to take a common caſe knife, and make inciſions acroſs it to the full depth of the knife's blade, at the diſtance of about one inch; and again croſsways in the ſame manner, the inciſions interſecting each other at right angles. The whey riſing through theſe inciſions is of a fine pale-green colour. The cheeſe-maker and two aſſiſtants then proceed to break the curd; this is performed by their repeatedly putting their hands down into the tub, the cheeſe-maker with the ſkimming-diſh in one hand breaking every part of it as they catch it, raiſing the curd from the bottom, and ſtill breaking it. This part of the buſineſs is continued till the whole is broken uniformly ſmall: it generally takes up about forty minutes, and the curd is then left covered over with a cloth for about half an hour to ſubſide. If the milk has been ſet cool together, the curd, as before mentioned, will be much more tender, the whey will not be ſo green, but rather of a milky appearance. The cheeſe-maker in this caſe, inſtead of the knife, has recourſe to the ſkimming-diſh, the edge of which ſhe holds perpendicular to the ſurface of the whey in the tub, and dips it gently an inch or two into the curd, turning it over, until the whole ſurface is thus turned. The breaking then proceeds as before; but a cautious and gentle mode of doing it is more neceſſary than in the former caſe. Rather more time, of courſe, is neceſſary for breaking down a cold than a hot cheeſe; but when ſufficiently broken, it is covered over, and left to ſubſide as before. After ſtanding about half an hour, as much whey is taken out of the tub into the braſs pans as conveniently may be, without taking any of the curd with it. The bottom of the tub is now ſet rather a tilt, the curd is collected to the upper ſide of it, a board is introduced of a ſemicircular form to fit looſely one-half of the tub's bottom. This board is placed on the curd, and a 60 lb. weight upon it, to preſs out the whey, which draining to the lower ſide of the tilted tub, is laded out into braſs pans. Such parts of the curd as are preſſed from under the board, are cut off with a knife, placed under the weighted board, and again preſſed. This is repeated again and again, the whey being conſtantly laded out as it drains from the curd. The whole maſs of curd is then turned upſide down, put on the other ſide of the tub, again preſſed, pared, and preſſed as before.

The board and weight being removed, the curd is cut into ſeveral pieces of about eight or nine inches ſquare, piled upon each other, and preſſed with the board and weight, repeating the cutting and piling, as long as any whey drains from it. It ſcarcely need be added, that the more gently the whole of the buſineſs in the tub is performed, the more perfect will be the ſeparation of curd and whey.

The next thing is to break the curd in a braſs pan. After being preſſed in the tub as long as any conſiderable quantity of whey drains from it, the curd is cut into nearly three equal portions; one of which is taken into a braſs pan, and is there, by two women, broken exceedingly fine. As ſoon as it is coarſely broken, a large handful of ſalt is added, which in the ſubſequent breaking is well mixed with the curd; that portion of curd, being ſufficiently broken, is put into a cheeſe-vat, which is placed to receive it on a cheeſe-ladder over the cheeſe-tub: the vat is generally furniſhed with a coarſe cheeſe-cloth. The ſecond and third portions of the curd are treated in the ſame manner, and emptied into the vat; ſometimes five or ſix times the quantity of ſalt is added to the middle portion of the curd; others ſalt all alike. The breaking takes up more or leſs time as the cheeſe was ſet together hotter or colder; half an hour is perhaps the longeſt time.

Thruſting or Hand-preſſing the Cheeſe into the Vat.

The curd, when put into the cheeſe-vat in its broken ſtate, is heaped above the vat in a conical form, to prevent it from crumbling down; the four corners of the cheeſe-cloth are turned over it, and three women, placing their hands againſt the conical part, gently but forcibly preſs it in nearly a horizontal direction, conſtantly ſhifting their hands when any portion of the curd is ſtarting from the maſs, and turning down and folding up the cloth as occaſion requires. As ſoon as the curd adheres together ſo as to admit it, a ſmall ſquare board, with a corner of the cloth under it, is put on the top of the conical part of the curd, with a 60 lb. weight upon the board. Several iron ſkewers are at this time ſtuck in the cone, and alſo through holes in the ſides of the vat. Several uſe a wooden lever to preſs down the cheeſe.

The employment of the women is now drawing out and putting in the ſkewers; thruſting and keeping together the portions of the curd, that the power of the weight or lever diſplaces. This operation is continued until the whey, which at firſt ran from it freely, begins to be diſcharged by drops; the weight and ſkewers are then removed, and one woman takes up the corners of the cloth, while the others break the curd half way to the bottom of the vat, as ſmall as they can.

Some people uſe a wooden or tin hoop, nine inches broad, inſtead of holding up the corners of the cloth during this breaking. After the upper half of the cheeſe is thus broken, a weight or other power is again applied to it, and is ſkewered and thruſted as before: at firſt the whey again runs freely, and the operation is continued as long as thoſe means will preſs out a drop of whey. Two of the women then take the four corners of the cloth, the ſkewers, &c. being removed; and the other woman lays hold of the vat which is drawn from the cheeſe; and after rinſing it in warm whey, and putting another clean cloth over the upper part of the cheeſe, it is returned inverted into the vat again, and, being placed on the ladder over the tub, is broken half way through, as before; thruſting, weighting, and ſkewering, &c. is repeated, and continued from two to four hours, or as long as a drop of whey can be extracted from the cheeſe.

Putting the Cheeſe into the Preſs.

When no more whey can be extracted by the aforeſaid means, the cheeſe is again turned into the vat and rinſed, as before, with warm whey. The cloth now made uſe of is larger and finer than the former, and is ſo laid that on one ſide it ſhall be level with the ſide of the vat, and on the other wrap over the whole ſurface of the cheeſe, and the edges put within the vat; thus perfectly incloſing the whole cheeſe in the cloth. In this ſtage of the buſineſs, the cheeſe is ſtill higher than the edge of the vat; and to preſerve it in due form recourſe is had to a tin binder or hoop, about three inches broad, which is put round the cheeſe on the outſide of the cloth, and the lower edge of the binder preſſed down within the vat, ſo low as that the upper edge of it may be level with the ſurface of the cheeſe. The cheeſe is then carried to the preſs; and a ſmooth ſtrong board being placed over it, the preſs is gently let down upon it; the uſual power of which preſs is about 14 or 1500 lb. weight.

As ſoon as the cheeſe is put into the preſs, it is well ſkewered; the ſkewers are of a ſtrong iron wire, 18 or 20 inches long, ſharp at the points, and turned with a bow at the other end. The vat and tin binder have holes to receive the ſkewers, eſpecially the binder, which holes are ſeldom more than one inch from each other. As the preſs ſtands cloſe to a wall, only one ſide of the cheeſe can be ſkewered at a time; therefore as many ſkewers are ſtuck in different directions as conveniently may, leaving as many holes unſkewered as are ſkewered, to give an opportunity of changing the holes. The buſineſs of ſkewering continues till the next morning at ſix o'clock, and in that time the oftener they are ſhifted the better: every ſecond time of ſhifting them the cheeſe is turned half way round in the preſs, to come at the other ſide of it. In half an hour from the time the cheeſe is firſt put into the preſs, it is taken out again and turned in the vat into another clean cloth. At this time the edges of the cheeſe are pared off, if they have become ſharp under the preſs; but as the vats are now uſually made with the angles rounded, the paring is rendered unneceſſary, the vat being wiped dry before the cheeſe is returned to it.

When the cheeſe is thus the firſt time taken out of the preſs, it is the cuſtom of ſome places to put it into warm, and in others into hot whey, where it ſtands an hour or more. It is then taken out, wiped dry, and after it has ſtood till cool, it is returned back to the preſs. This is done with a view to hardening its coat, that it may ſtand the better. At ſix o'clock in the evening the cheeſe is again turned in the vat into another clean cloth. At this and the former turning, ſome dairy-women prick the upper ſurface of the cheeſe all over, an inch or two deep, before it is replaced under the preſs, with a view of preventing bliſters. At ſix o'clock in the morning it is again turned in the vat, with a clean cloth as before. The ſkewers are now laid aſide. When the next day's cheeſe is ready for the preſs, the former one is again turned in the vat with a clean cloth, and put under another preſs. At ſix o'clock in the evening, and at ſix the morning following, it is again turned in the vat, uſing at theſe two laſt turnings two cloths finer than thoſe before uſed, in order that as little impreſſion as poſſible from the cloth may remain on the coat of the cheeſe.

Salting the Cheeſe.

Four or five days after the cheeſe has been under the preſs, a freſh fine cloth is put under it, which ſerves only as a lining to the vat, and is not turned over the upper ſurface of the cheeſe, as has been hitherto the caſe. It is then taken and placed nearly midſide deep in brine in a ſalting tub; the upper ſurface of the cheeſe being covered all over with ſalt. It ſtands generally about three days in the ſalting tub; is turned daily, and each turning well ſalted, the cloth being twice changed in the time. The cheeſe is then taken out of the vat, and in lieu of which a wooden girth or hoop is made uſe of, equal in breadth nearly to the thickneſs of the cheeſe: in this it is placed on the ſalting-benches, where it ſtands about eight days, being well ſalted all over, and turned each day. The cheeſe is then waſhed in luke-warm water; and after being dried with a cloth, it is placed on the drying-benches, where it remains about ſeven days: it is then waſhed in warm water, as before, with a bruſh, and wiped dry with a cloth: after it has ſtood about two hours from this waſhing, it is ſmeared all over with about two ounces of ſweet whey butter, and then placed in the warmeſt part of the cheeſe-room.

Cheeſe-Room.

During the firſt ſeven days it is rubbed every day well over, and generally ſmeared with ſweet whey butter. Afterwards a circular ſpace is left unrubbed of four or five inches diameter, in the centre of each ſide the cheeſe, which, as long as it is afterwards kept, is, or ſhould be, turned daily, and rubbed three times a week in ſummer and twice in winter. Scraping the rind ſhould be rendered unneceſſary by frequent cleanings. In a warm room the coat will be eaſily prevented from riſing. Theſe cheeſe-rooms are commonly placed over the cow-houſes, and this is done with a view to obtain that moderate and neceſſary degree of temperature ſo eſſential to the ripening of cheeſe, to which the heat ariſing from the cattle underneath is ſuppoſed very much to contribute. The moſt deſirable covering for a cheeſe-room, as contributing to that temperature ſo much deſired, is thatch, for reaſons that muſt be obvious. Before the cheeſe is brought into the rooms, the floors are moſtly well littered with what the farmers here call *ſniddle*, though wheat ſtraw is frequently made uſe of for this purpoſe, but the knots of it are apt to leave an impreſſion on the cheeſe. The afterneath of graſs well dried ſeems to be a good ſubſtitute for ſniddle.

XL. *On the Antiquity and Invention of Fire Engines.* By *Profeſſor* BECKMANN.

AN examination of the invention of forcing pumps I ſhall leave to thoſe who may be diſpoſed to write a hiſtory of hydraulics; I ſhall however obſerve, that, on the teſtimony of Vitruvius *, they are generally aſcribed to Cteſibius; that on this account they are uſually called *machinæ Cteſibicæ*; and that Cteſibius lived at Alexandria about the time of Ptolemy Philadelphus and Ptolemy Euergetes I.; conſequently, about two hundred years before the Chriſtian æra. My object here is to lay before the reader what information I have been able to collect reſpecting the queſtion, At what time did theſe machines begin to be employed for extinguiſhing fires?

To render them fit for this purpoſe, it was neceſſary that ſome alteration ſhould be made in their conſtruction, as at firſt they were employed only in the raiſing of water. To render them fit for extinguiſhing fires, it was neceſſary that the water ſhould be forced through the upper aperture to as great a height as poſſible; whereas, for the former purpoſe, it is enough if it flows from the upper aperture in ſufficient quantity into the reſervoir, from which it is to be conveyed to the place of its deſtination. In this caſe, the apparatus required for a machine to be uſed as a fire engine would be a defect; as the power which communicates to the water its velocity might be employed with more advantage to raiſe a larger quantity.

In my opinion, it is highly probable that Cteſibius had an idea of converting his forcing pump into a fire engine; for his ſcholar Hero of Alexandria ſpeaks expreſsly of this application, and deſcribes the method of conſtructing a kind of engine with two cylinders †; but it is doubtful whether this application of the forcing pump ſoon became common, and whether the antient Romans were in poſſeſſion of this uſeful machine. All that I know on the ſubject is as follows:

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* Lib. x. c. 12. p. 347. compare alſo lib. ix. c. 9. p. 321.

† In that work entitled Πνευματικα, or *Spiritalia*. It may be found with a Latin tranſlation in the *Veterum Mathematicorum Opera*, Pariſiis, 1693. folio. In p. 180, the following paſſage occurs: Οἱ δὲ σίφωνες, οἷς χρῶνται εἰς τοὺς ἐμπρησμοὺς, κατασκευάζονται οὕτως. Siphones, quibus utuntur ad incendias, hoc modo conſtruuntur. The annexed drawing repreſents an engine with two cylinders. Reſpecting the other editions of this work ſee *Fabricii Bibliotheca Græca*, vol. ii. p. 593, where the Latin tranſlation which I have in my poſſeſſion is omitted. It is entitled *Heronis Alexandrini Spiritalium Liber. A Federico Commandino Urbinate ex Græco nuper in Latinum converſus.* Pariſiis apud Aeg. Gorbinum, 1583. 4to. This edition ends with the words *et magis continuata converſio fiat*, which in the Paris edition ſtand in p. 230. It wants alſo the end of the Paris edition from the words, *Quomodo animal diviſum et bibat*, to *et tunc oblato poculo*

Pliny the younger informs the emperor Trajan in one of his letters, that the city of Nicomedia in Bithynia had been almoſt entirely deſtroyed by fire; and that the devaſtation had been greater becauſe a violent ſtorm had taken place during the conflagration, and becauſe there was no apparatus in the city for extinguiſhing fires *. The *ſipho* here mentioned by Pliny ſeems, beyond doubt, to have been the fire engine of Cteſibius; though among the other articles we are to underſtand water conduits, and pipes employed for conveying water into the city. I muſt however confeſs that the word *ſipho* ſignifies alſo pipes of this kind: and particularly in a paſſage of Strabo †, where he ſpeaks of the ſubterranean conduits at Rome, and ſays, that almoſt all the houſes had ciſterns, *ſiphones*, or water pipes, and running ſtreams. But Pliny mentions at the ſame time water buckets, which are to be conſidered as an appendage abſolutely neceſſary to fire engines. It is alſo hardly poſſible to believe that a city ſituated as Nicomedia was, cloſe to a bay ‡, ſhould be in want of water.

But I can produce, and I believe I am the firſt to do ſo, for I do not find that it has ever been mentioned by any other writer, a very ſtrong proof from a cotemporary author. Apollodorus the architect, who was employed by the emperor Trajan in conſtructing the celebrated bridge on the Danube, and in completing ſome extenſive works at Rome; and who,

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pondo bibet. More information reſpecting Hero may be found in *Heronis Cteſibii Belopoeeca, hoc eſt Telifactiva, Bernardino Baldo Urbinate Guaſtallæ Abbate Illuſtratore et Interprete. Item Heronis Vita eodem Auctore.* Auguſtæ Vindel. 1616. 4to. See alſo *Heronis Alex. Vita, et Scripta et quædam Inventa, Præſide R. A. Wagnero, Auctore J. A. Schmidt.* Helmſtadii 1714. 4to.

* *Epiſt.* 42. lib. x. Incendium latius ſparſum; primum violentia venti, deinde inertia hominum, quod ſatis conſtat, otioſos et immobiles tanti mali ſpectatores perſtitiſſe, et alioqui nullus uſquam in publico ſipho, nulla hama, nullum denique inſtrumentum ad incendia compeſcenda, et hæc quidem ut jam præcepi, parabantur.

† Lib. v. edit. Almel. p. 360. Τοσουτον δ᾽ εςι το εισαγωγιμον ὑδωρ δια των ὑδραγωγειων, ὡςε ποταμους δια της πολεως και των ὑπονομων ρειν, ἁπασαν δε οικιαν σχεδον δεξαμενας και σιφωνας, και κρουνους εχειν αφθονους. Tantum aquæ per aquæductus in urbem derivatur, ut flumina per urbem et cloacam labantur, et quælibet propemodum domus ciſternas, tubulos ac canales habeat copioſos. Compare Lipſius *de Magnitudine Romana*, Antwerp 1598. fol. lib. iii. c. 11. p. 181.

‡ *Plinius* v. cap. ult. Eſt in intimo ſinu (Aſtaceno) Nicomedia Bithyniæ præclara.

as we are told by Dio Caſſius, was put to death by Trajan's ſucceſſor, Adrian, on account of a ſneering anſwer which he gave to that prince, tells us, in the remains of his work on warlike machines, what methods are to be employed when the upper part of an edifice is on fire, and the machine called *ſipho* is not at hand. In this caſe, ſays he, leather bags filled with water muſt be faſtened to long pipes in ſuch a manner that, by preſſing the bags, the water may be forced through the pipes and thrown on the place which is on fire *. The *ſipho*, therefore, was a machine by means of which water could be thrown to a height on places on fire, and which could not be reached in any other manner.

That in the fourth century, at leaſt, a fire engine properly ſo called was underſtood by the term *ſipho*, is fully proved by Heſychius †; as alſo by Iſidore, who lived in the beginning of the ſeventh century ‡. The latter remarks, that ſuch engines were employed in the eaſt for extinguiſhing fires; and thence there is almoſt reaſon to conclude that they were not then uſed in the weſt.

It now remains to determine the queſtion, at what time ſuch apparatus for extinguiſhing fires began to be employed at Rome. From the regulations for guarding againſt accidents by fire, and in regard to extinguiſhing fires, which occur in the Roman code §, one might be induced to ſuppoſe that this capital muſt have been furniſhed with thoſe neceſſary machines, of the want of which, in a provincial city,

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* *Poliorcetica*, p. 32. in Veterum Mathematic. Oper. Κἂν που ακρωτηριον καιηται δυσεπιβατον, μη ῃ δε οργανον ὁ καλειται σιφων, καλαμοι παλιν τετρημμενοι ὥσπερ οἱ των ιξευτων, ἁρμοζονται, οπου δει φερειν αυτους ὑδωρ, ασκοι τε πληρεις πιεζομενοι εκθλιβωσι δι' αυτων επι τον καιομενον τοπον. Si forte extremitas aliqua, ad quam difficilis ſit aſcenſus, ardeat, nec præſto ſit inſtrumentum illud quod vocatur ſipho, arundines rurſus perforatæ, cujuſmodi ſunt aucupum, aptantur in iis locis quo aquam ferre eas oportet, et utres aqua pleni preſſi aquam ejaculantur in locum qui igni conſumitur.

† Σιφων οργανον τι εις προεσιν ὑδατων εν τοις εμπρησμοις. Inſtramentum ad ejaculandas aquas adverſus incendia.

‡ *Origines* xx. 6. Siphon vas appellatum quod aquis ſufflando fundat; utuntur enim hoc Orientales. Nam ubi ſenſerint domum ardere, currunt cum ſiphonis plenis aquis et extinguunt incendia, ſed et cum aras expreſſis ad ſuperiora aquis emundant. In many places hand engines are uſed in the like manner for waſhing the windows of the upper ſtories.

§ See *Digeſt.* i. tit. 15. where it is recommended to all perſons to keep water by them in their houſes. Ut aquam unuſquiſque in cœnaculo (the upper ſtories?) habeat, jubetur admonere. Alſo *Digeſt.* xlvii. tit. 9. many things relating to this ſubject may be found in *L. A. Hambergeri Opuſcula*, Jenæ et Lipſ. 1740. 8vo. p. 12. in the diſſertation *de Incendiis.* The following diſſertations, which I have not ſeen, contain alſo information reſpecting the police regulations of the Romans in regard to fires. G. C. Marquarti *de cura Romanorum circa Incendia*, Lipſ. 1689. 4to. E. Ottonis *Diſſert. de Officio Præfecti Vigilum circa Incendia*, Ultrajecti, 1723.

Pliny complains, and which he had himſelf ſupplied. This conjecture, however, I cannot confirm; and antient as well as modern examples prove that the police regulations eſtabliſhed in ſmall towns are not always to be found in capitals. Antioch and ſeveral cities had ſtreet-lanterns, which were wanting in the proud Rome. But what excites ſome doubt is, that theſe fire engines are never mentioned in the Roman laws, nor in the accounts of the fires which frequently happened at Rome. When a misfortune of this kind happens at preſent, it is always mentioned whether a ſufficient number of engines were preſent, and what effect they produced; and Pliny, indeed, does not omit them in the ſhort account he gives of the fire at Nicomedia.

One paſſage only in Ulpian is commonly produced as a proof that, in his time, there were fire engines at Rome. Where he enumerates thoſe things that belong to a houſe when ſold, he mentions, beſides other articles, the *ſiphones* * employed for extinguiſhing fires. But if this term here means a fire engine, the paſſage appears to prove too much; for, in that caſe, we muſt believe that each houſe then was furniſhed with an engine of its own: if ſo, theſe engines muſt have been ſmall portable ones, ſuch as are common in many houſes at preſent; and the paſſage therefore cannot be adduced as a proof of public engines, ſuch as Pliny regrets the want of at Nicomedia. But it is more probable that Ulpian only alludes to thoſe *ſiphones* which according to Strabo's account were to be found in every houſe at Rome; that is, pipes which conveyed water into them for domeſtic purpoſes. From the total want of fire engines, or the imperfect manner in which they were conſtructed, what Seneca ſays muſt have been very true, that the height of the houſes at Rome rendered it impoſſible to extinguiſh the fires which happened in that city †. That the houſes there were exceedingly high, and the ſtreets, bridges, and highways, very narrow, is well known. Archenholz [*] and others are of opinion, that the houſes were raiſed to ſuch a height on account of the great heat in that warm climate; but the principal cauſe was that aſſigned by Vitruvius [†], and which ſtill produces the ſame effect. For want of room on the earth, the buildings were extended towards the heavens; ſo that at laſt the greateſt height of an edifice was fixed by a law at 70, and afterwards at 60 feet. In Hamburgh at preſent, where ground is valuable, and ſtill becoming more ſo, moſt of the houſes are 60 feet, and ſome 70 feet in height; and how much this height, notwithſtanding the perfection to which fire engines have been brought, renders the extinguiſhing of fires difficult, and often impoſſible, is ſhown by the melancholy inſtance of Gera, where the houſes are built lower. With Neubert's engine, which was tried at Hamburgh in 1769, eight fire-men raiſed 11½ cubic feet of water to the height of about 62 or 63 feet. The beſt Engliſh engines can throw water to the height only of from 32 to 33 feet [‡].

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* *Digeſt.* xxxiii. 7. 18. Acetum quoque, quod extinguendi incendii cauſa paratur, item centones, ſiphones, perticæ quoque et ſcalæ, et formiones et ſpongias et hamas et ſcopas contineri, plerique et Pegaſus aiunt. Alexander ab Alexandro, whoſe authority however is not deciſive, underſtands here engines. *Dier. Genial.* v. 24. p. 342. Siphones vero, fiſtulas follibus junctas ſeu machinas hydraulicas, quibus agitatis, at ſuperiora ædium exhauriunt aquam, qui etiam organa pneumatica dicti. Theſe *fiſtulæ follibus junctæ*, which no commentator has explained, are thoſe tubes which were before propoſed by Apollodorus.

† *Controverſ.* ix. lib. 2. p. 153. Ædes, quas in tantum exſtruxere, ut domus ad uſum et munimentum paratæ, ſint nunc periculo, non præſidio; tanta altitudo ædificiorum eſt, tantæque viarum anguſtiæ, ut neque adverſus ignem præſidium, neque ex ruinis ullum ullam in partem effugium ſit.

In the Eaſt, ſuch engines were employed not only to extinguiſh fires, but to excite conflagrations. The Greek fire, invented in the year 678, by Callinicus, an architect of Heliopolis, called afterwards Balbec, the uſe of which was continued in the Eaſt till the year 1291 at leaſt [§], and which certainly was liquid [||], was employed in many ways, but chiefly on board ſhips, being thrown, by means of large fire engines, into the ſhips of the enemy. Sometimes this fire was kindled in certain veſſels which might be called *fire ſhips*, and which were introduced among the hoſtile fleet [¶]; ſometimes it was thrown upon the enemy from jars and other veſſels with machines of various kinds [**]; and ſometimes the ſoldiers ſquirted it from hand engines, or, as appears, by blowing it through tubes: but neither theſe hand engines nor ſuch tubes could have been the machines employed for projecting it from the fore part of ſhips againſt thoſe of the enemy. They were conſtructed of copper and iron; the extremity of them had the appearance ſometimes of the open mouth and jaws of a lion or other animals; they were painted, and even gilded; and it appears that the fire could be thrown from them to a great diſtance.

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[*] England und Italien, Lipſ. 1785. 8vo. ii. p. 216.

[†] Lib. ii. c. 8. p. 68. In ea majeſtate urbis et civium infinita frequentia, innumerabiles habitationes opus fuit explicare. Ergo cum recipere non poſſet area plana tantam multitudinem ad habitandum in urbe, ad auxilium altitudinis ædificiorum res ipſa coegit devenire.

[‡] This is a miſtake of the author. Some Engliſh engines can throw water to a much greater height.—Edit.

[§] Hanovii Diſquiſitiones; Gedani 1750. 4to. p 65.

[||] *Annæ Comnenæ Alexiad.* lib. xvi p. 385. Πυρ ὑγρον: the ſame appellation occurs in Theophanes Chronographia.

[¶] Theophanes, p. 294 and 352.

[**] Such a machine is called by Joinville in *Hiſtoire de S. Louis* IX. Paris 1668. fol. p. 39. ung engin, qu'ils appelloient la Parriere, par lequel engin ils nous gettoient le feu Gregois à planté. Does not the following paſſage alſo in *Leonis Tactica*, Lugd. Bat. 1612. 4to. p. 347. allude to the Greek fire? though Meurſius thinks that the author ſpeaks of unſlaked lime, which, however, could not produce the deſcribed effect: Χυτρας και αλλ' ιυς ασβεστου πληρες, ὡν ῥιπτομενων και συντριβομενων, ὁ το[illegible]ου ατμος:

Theſe machines are called expreſsly by various authors *ſpouting engines*. Thus Johannes Cameniata, ſpeaking of the taking of his native city Theſſalonica by the Saracens in the year 904, ſays that the enemy threw fire into the wooden works of the beſieged, which was blown into them by means of tubes, and thrown from other veſſels *. This paſſage, which I do not find quoted in any of the works that ſpeak of the Greek fire, proves, at the ſame time, that at the beginning of the tenth century the Greeks were not the only poſſeſſors of the art of preparing this fire, the forerunner of our gunpowder. The emperor Leo, who about this time wrote his Art of War, recommends the uſe of ſuch tubes on the fore part of ſhips, and to be placed under the ſhelter of a proper covering †: he mentions the ſquirting of the Greek fire in two more paſſages ‡. In a word, people may have eaſily conceived the idea of diſcharging the Greek fire by means of a forcing pump, becauſe the uſe of this machine for extinguiſhing fires was known there long before the invention of Callinicus.

At what period the cities in Germany began to be furniſhed with fire engines I have not been able to determine.

ατμος συμπνιγει και σκοτιζει τους πολεμιους, και μεγα εμποδιον γινεται. Ollas calce viva plenas alii injiciunt, quibus contractis, calcis vivæ pulvis diſſipatus ſuffocat et ſtrangulat hoſtes, et magno ad prælianduum impedimento eſt.

[*] In Leonis Allatii *Συμμικτα*. Coloniæ Agripp. 1653. 8vo. p. 389. *Πυρ τε δια των σιφωνων τω αερι φυσησαντες, και τινα αλλα σκευη'. . .* ignem ſiphonibus efflantes, et alia vaſa, flammarum plena, in murum conjicientes.

[†] Cap. xix. § 6. p. 322. *Εχετω δε παντως τον σιφωνα κατα την πρωραν εμπροσθεν χαλκῳ ημφιεσμενον, ὡς εθος, δι' ου το εσκευασμενον πυρ κατα των εναντιων ακοντισει* (according to the reading in *Fabricii Bibliotheca Græca*, vi. p. 373.): In prora ſiphonem aere obtectum de more habeas ad ignem in hoſtes ejaculandum, et celſe ſupra ſiphonem pſeudopatium ex aſſeribus confectum et aſſeribus circumtectum, in quo viri ad bellandum inſtructi ſint.

[‡] *Pag.* 344. Interdum frontem claſſis directam inſtrues, ut ubi uſus erat in hoſtium proras irruat, et ſiphonibus ignem ejicientibus naves illorum incendat. *Δια του πυρος των σιφωνων.* *P.* 346. Multæ molitiones excogitatæ ſunt; cujus generis ſunt, ignis cum tonitru et fumo ignito per ſiphones emiſſus, et incendens naves. *Οιον τοτε εσκευασμενον πυρ μετα βροντης και καπνου προτερον δια των σιφωνων πεμπομενον.*

In my opinion they had regulations reſpecting fires before they had engines; and the former do not ſeem to be older than the beginning or middle of the ſixteenth century. The oldeſt for the city of Franckfort on the Mayn, with which I am acquainted, are of the year 1460[*]. The firſt general order in Saxony reſpecting fire, is that of duke George in the year 1521[†]. The firſt for the city of Dreſden, and which was to ſerve for the whole country, is of the year 1529[‡]. In many cities, the firſt ordinances publiſhed by government for the purpoſe of preventing danger from fire, may be found perhaps among the regulations in regard to building, which appear to be ſomewhat older than the particular ones reſpecting fires. At Augſburg an expreſs regulation in regard to building was drawn up and publiſhed ſo early as 1447[§]. On turning over the old chronicles it is obſerved that great fires began to be leſs frequent in the ſixteenth century; a circumſtance which beyond all doubt muſt be aſcribed to the improvements in building[‖], the regulations made by government for the guarding againſt fires, and the introduction of apparatus for extinguiſhing them. But by the invention of fire engines every thing in this reſpect has been ſo much changed, as to render it neceſſary to reviſe and entirely alter the old regulations made in regard to fires; and it is not impoſſible that the firſt mention of public fire engines will occur in the new regulations reſpecting fires made in the ſixteenth and ſeventeenth centuries.

Mr. Von Stetten has remarked, that the firſt mention of fire engines at Augſburg occurs in the building accounts of the city for the year 1518. They are called there *inſtruments for fires, water ſyringes uſeful at fires*[¶]; and theſe names ſeem to announce that the machine was then in its infancy. At that time they were conſtructed by Anthony Blatner, a goldſmith of Friedberg, but who in the above year was a citizen of Augſburg. From the obſervation added, that the wheels and poles (levers) were made by a wheel-wright, and by the largeneſs of the ſum put down in the account, there is reaſon to conclude that theſe were not hand engines, but large compound machines. In that reſpectable dictionary *Maalers Teutſchſpraach*, printed at Zurich in 1561. 8vo., I find *feuerhaken* (fire hooks), and *feuerleitern* (fire ladders); but no machines for extinguiſhing fires, nor any where a fire engine.

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[*] A paſſage from it is quoted in Orth's *Anmerkungen uber die erneuerte Reformation der Stadt Frankfurt* 1751. 4to. iii. 404.

[†] It is intitled Begreyff der Fewer Ordenunge, four pages folio. It is wanting in the *Codex Auguſteus*, but its contents may be found in Canzler und Meiſner für ältere Litteratur und neuere Lectüre, Lipſ. 1785. 8vo. iii. 3. p. 97.

[‡] Wecken's Beſchreibung und Vorſtellung der Reſidenz Dreſden, p. 482.

[§] Von Stetten Kunſtgeſchichte der Stadt Augſburg, i. p. 87.

[‖] Thus, in the year 1466 ſtraw thatch was forbidden at Franckfort; and in the year 1474 roofs conſtructed of ſhingles. *Lerſner*, ii. p. 22.

[¶] Kunſtgeſchichte der Stadt Augſburg, ii. p. 112.

In the year 1655 the jeſuit Caſpar Schott was ſtruck with admiration on ſeeing at Nuremberg an engine, made there by John Hautſch. It ſtood on a ſledge ten feet in length and four in breadth. The veſſel that contained the water was eight feet in length, four in height, and two in breadth. It was put in motion by twenty-eight men, and forced a ſtream of water an inch in diameter to the height of eighty feet, and conſequently above the houſes. The whole machine was drawn by two horſes. Hautſch is ſaid to have cauſed an engraving to be made of this machine, which he circulated, with an offer of conſtructing ſimilar ones at a moderate price, and teaching the uſe of them. He refuſed to ſhow Schott the internal conſtruction of his machine; but the latter readily conceived it. From what Schott ſays, it evidently appears that the cylinders did not ſtand perpendicular, but lay in a horizontal direction in the box; ſo that the piſtons moved horizontally, and not vertically as at preſent. Vertical cylinders, therefore, ſeem to be among the improvements ſince made to this engine. Schott adds, that this invention was not new, it had been known in other cities; and he himſelf forty years before, conſequently in 1617, had ſeen one, but much ſmaller, in his native city *. Schott, as is well known, was born at Konigſhofen, not far from Würzburg, in 1608. George Hautſch, the ſon of the former artiſt, conſtructed ſuch engines alſo; and probably made ſome improvement in them; for Wagenſeil † and others aſcribe to him the invention.

The firſt regulations at Paris reſpecting fires, as far as is known, relate to ſuppreſſing thoſe incendiaries who, under the name of *boutefeux*, occaſioned great devaſtation in the capital as well as in the provinces ‡. Paris ſeems to have obtained the firſt fire engines in 1699: in that year, at leaſt, the king gave to Dumourier Duperrier an excluſive privilege

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[*] *Magia Univerſalis*, p. iii. lib. 6. p. 510; and in *Paſchii Invent. Nov.-antiq.* Lipſ. 1700. 4to. p. 668. The Magia Univerſalis was printed in 1657. See alſo Doppelmayr, p. 301, who ſays that the water was raiſed to the height of a hundred feet.

[†] De Civitate Norimbergenſi, p. 153. Marpergers Geöfnetes Manufakturhaus. Hamburg 1707. 12mo. p. 210. Doppelmayr, p. 303.

[‡] Continuation du Traité de la Police par Delamare, Paris 1738. fol. p. 137.

to conſtruct theſe machines, which were called *pompes portatives*; and he engaged for a certain ſum to keep in repair the ſeventeen engines which had been purchaſed for the uſe of the city, and to provide and pay the neceſſary workmen. In the year 1722 the number was increaſed to thirty, which were diſtributed in ſeveral quarters of the city; and the contractors received annually at that time 20.000 livres. The city, however, beſides the thirty royal engines, had others which belonged to the *hotel de ville*, with which the ſieur Duperrier had nothing to do [*].

In the middle of the ſixteenth century theſe machines were certainly ſtill very imperfect. They had neither an air chamber nor buckets, and required a great many men to work them. They conſiſted only of a ſucking pump and forcing pump united, which forced the water out in ſpurts, and with continual interruptions. Such machines in moving the lever experience a ſtoppage, during which no water is forced out; and, as the pipe is fixed, it cannot convey the water into a houſe on fire, though it may reach a fire at no great diſtance where there are doors and windows to afford it a paſſage. At the ſame time, the workmen are liable to be cruſhed by the fall of the houſes, and on that account muſt keep from them. Hautſch, however, had given to his engine a flexible pipe, which could be turned to any ſide as might be neceſſary; but not an air-chamber, which Schott certainly would have deſcribed. Even in Belidor's time, the fire engines in France were of no other kind. In the year 1760 theſe improved machines were uſed only in England; at leaſt profeſſor Buſch [†] concludes ſo from this circumſtance, that Ferguſon conſiders Newſham's engine, which threw the water out in a continued ſtream, as a new invention.

It is not with certainty known who firſt conceived the idea of conſtructing an engine with an air-chamber, in which the included air, by compreſſing the water, forces it out in a continued ſtream. According to a conjecture of Perrault, it would appear that Vitruvius ſeems to ſpeak of a ſimilar conſtruction; but Perrault himſelf acknowledges that this obſcure paſſage [‡] can be explained in a different manner. The air-chamber in its action has however ſome reſemblance to Hero's fountain, in which the air, being compreſſed by the water, forces the water out [§].

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[*] Continuation du Traité de la Police, &c. p. 157.

[†] Verſuch einer Mathematik zum Nutzen und Vergnugen, Hamburg 1791. 8vo. p. 396.

[‡] Lib. x. c. 12.

[§] Spiritalia, xxxvi. p. 85.

I can find no older engine with an air-chamber than that deſcribed by Perrault, and of which he has given a figure. He ſays it was preſerved in the king's library at Paris; that it was employed for throwing the water to a great height during fires; that it had only one cylinder, and yet threw out a continued jet of water. He neither mentions the period of the invention, nor the name of the inventor; and I can only add that his book was printed in 1684 *. The principle however ſeems to have been pointed out before by Mariotte, who by ſome is, on this account, conſidered as the inventor; but he does not appear to have entertained any idea of a fire engine, at leaſt he makes no mention of it †.

It is certain that the air-chamber, at leaſt in Germany, came into common uſe after it had been applied to theſe engines by Leupold, who conſtructed and ſold a great many of them. In the year 1720 he publiſhed an account of them in a ſmall work conſiſting of a few ſheets in quarto: but at firſt he kept the conſtruction a ſecret; and thoſe engines which he ſold conſiſted of a ſtrong copper box, entirely ſhut and ſtrongly ſoldered. It weighed only ſixteen pounds; occupied very little ſpace; had but one cylinder; and one man by means of it could throw the water, without interruption, to the height of from twenty to thirty feet ‡. About the year 1725, Du Fay ſaw one of theſe machines of Leupold at Straſburgh. He conceived, as he ſays, its conſtruction, and made it known in the Tranſactions of the Academy of Sciences for 1725. It is very ſingular that Du Fay makes no mention of Mariotte, nor of the engine preſerved in the king's library at Paris. Leupold however, ſo early as the year 1724, gave a figure and deſcription of his engine in his *Theatrum Machin. Hydraulicarum*, vol. i. p. 120. tab. 45. fig. 2., which was not known to the French author.

An uſeful improvement of this engine conſiſts in the hoſe added to the engine, which can be lengthened or ſhortened, and to which the fire-pipe is applied, ſo that the perſon who directs it can approach the fire with much leſs danger. This invention, as is well known, belongs to two Dutchmen, both named John van der Heide [*], who were inſpectors of the apparatus for extinguiſhing fires at Amſterdam. The firſt public experiments made with them took place in the year 1672, and were attended with ſo much ſucceſs, that at a fire next year the old engines were uſed for the laſt, and the engines with moveable hoſe for the firſt, time. In the year 1677 the inventor obtained an excluſive privilege to conſtruct engines according to their principle for twenty-five years. In the year 1682 a ſufficient number of theſe engines were diſtributed in the different quarters of the city, and the old engines were entirely laid aſide. In the year 1695 there were in Amſterdam more than ſixty of theſe engines, the ſix of which neareſt the place where a fire broke out, were required to be preſent to give aſſiſtance. In a few years they were common in all the towns of the Netherlands.

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* I poſſeſs only the ſecond enlarged edition of his *Architecture de Vitruve*, large folio, where the account is to be found, p. 318. Whether it be in the firſt edition of 1673, I do not know. Daniel Bernoulli has given a figure and deſcription of it in his *Hydrodynamica*, Argentorati 1738. 4to. p. 172. fig. 51.

† The paſſage may be found in *Traité du Mouvement des Eaux*, p. 4. diſc. i; and in *Oeuvres de Mariotte*, Leide 1717. 2 vol. 4to. i. p. 445. fig. 59.

‡ An extract from his work may be ſeen in *Breſlauer Sammlungen*, verſuch vi. p. 2035, and verſuch vii. p. 374.

All theſe circumſtances have been related by the inventor himſelf in a particular work, which, on account of the excellent copper-plates in it, has become exceedingly ſcarce [†]. The firſt ſeven of theſe repreſent ſome dreadful conflagrations, at which the old engines were uſed without producing much effect. Among them is the conflagration of the ſtadt-houſe of Amſterdam in the year 1652. The twelve following plates are repreſentations of fires which were extinguiſhed by means of the new engines, and exhibit at the ſame time the various ways in which they could be employed with advantage. According to an annexed account, it appears that the city of Amſterdam loſt by ten fires, at which the old apparatus was uſed, 1,024,130 florins; but during the ten following years, when the new engines were introduced, only 18,355 were loſt by forty fires; ſo that the annual ſaving by this invention was 98 per cent.

The internal conſtruction of theſe engines has no where been repreſented, nor do I remember to have read a paſſage in any author which proves that they were furniſhed with an air-chamber, though in the privilege they are called

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[*] In the privilege, however, they are called *Jan ende Nicolaas van der Heyden.*

[†] Beſchryving der nieuwlijks uitgevonden en geoctrojeerde Slang-Brand-ſpuiten, en haare wijze van Brand-Bluſſen, tegenwoordig binnen Amſterdam in gebruik zijnde.—Door der zelver Inventeur Jan van der Heide en Jan van der Heide de jonge, generaale Brandmeeſters der Stad Amſterdam. Amſterd. 1690. To this ſubject belongs alſo the following work, a copy of which is in our library: Beſchryving der Brand-orders, en wijze van Brandbluſſen, tegenwoordig binnen Amſterdam in gebruik zijnde, door Jan van der Heide, en Jan van der Heide de Jonge, generaale Brandmeeſters der zelver Steede. Amſterd. 1695; one ſheet and a half 4to. with four plates, and a deſcription of them. One of theſe deſcriptions is inſerted in the Leipſic Fire Regulations for the year 1759. 4to.

*ſpouting engines**. The deſcription even of the leather pipe, or hoſe, is very ſhort and defective; probably with a view to render it difficult to be imitated: it is only ſaid that it was made in a peculiar manner of leather; ſo that it was not only thick, but could withſtand the force of the water†.

The conveyer or bringer was invented alſo in Holland about the ſame time. This name is given at preſent to a box which has on one ſide a ſucking pump, and on the other a forcing pump. The former ſerves to raiſe the water from a ſtream, well, or other reſervoir, by means of a ſtiff hoſe, the extremity of which is furniſhed with a metal ſtrainer to prevent dirt from entering it, and which is kept ſuſpended above the mud by means of a piece of cork. The forcing pump forces the water thus drawn up, through another hoſe, into the engine, and thus renders unneceſſary the labour of ſupplying it with water by means of buckets.

At firſt, indeed, this machine was very ſimple. It conſiſted of a leather hoſe ſcrewed upon the engine, the end of which widened into a kind of bag ſupported near the reſervoir, and kept open by means of a frame; and the labourers poured water into it from buckets. The Van der Heides, however, had for this purpoſe employed a pump which they called a *ſnake pump*, and by which they could raiſe the water upwards of a thouſand feet. But how it was conſtructed I no where find mentioned: from the figure, I ſhould almoſt conjecture that they uſed only a cylinder with a lever. They ſometimes alſo placed a portable pump in the water, and thus raiſed water into the annexed hoſe, which conveyed it to the engine. Every leather pipe employed for conducting water they called a *water ſnake*; and theſe were not made, as the hoſe of the fire engine, of leather, but of ſail-cloth: they however ſay, that for this purpoſe the ſail cloth required a peculiar preparation‡, which conſiſted in making it water proof by applying to it a certain kind of cement. The hoſe alſo through which the water is conveyed muſt be ſtiffened, and diſtended by metal rings; otherwiſe the external air on the firſt ſtroke of the pump would compreſs the hoſe ſo that it could admit into it no water. It is here ſeen that the uſe of ſail-cloth hoſe is not ſo modern an invention as ſome have ſuppoſed. That our apparatus for conveying water to the fire engine is much more ingenious and convenient muſt be allowed; but I would ſtrongly recommend that in all cities there ſhould be pumps or running wells of water, to the ſpout of which the one end of hoſe might be fixed, while the other is ſcrewed upon the engine. The Van der Heide reckoned it an advantage in their invention, that this apparatus rendered it unneceſſary to have leathern buckets, which are expenſive; or at leaſt leſſened their number, as well as that of the workmen.

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* Slang-Brand-Spuyten, beſtaande in een geduirig ſtralende Brand-Spuit, met een buygelijke buys daar aan, om haar gedaante een Slang genaamd, die men kan verlangen naar eyſch ende welgevallen.

† Page 6. Slang, of Spuit-Slangen van leer gemaakt, dat op een byzondere wyze bereid en zaamen gevoegt word om dicht, beſtendig en tegen't geweld van't water beſtand te zyn. In the French explanation of the plates theſe new engines are called *pompes à boyaux*.

‡ Page 5. Water-Slang, zynde een lange en buiggelyke buis, van zeker ſoort van doek, hier toe byzonderlyk bereid, gemaakt.

From this information, the truth of which cannot be doubted, we may readily believe that engines with hoſe were not invented in the year 1697 by Gottfried Fuchs, fire director at Copenhagen, as was however publicly aſſerted in 1717; with this addition, that the invention was ſoon after employed in Holland and at Hamburgh *. Fuchs ſeems only to have introduced into Denmark the invention of the above Dutchmen at the time of the great fire on the 19th of April 1689, when the opera houſe at Amalienburg, with more than 130 perſons, as well as the beautiful palace of Amalienburg, were deſtroyed. We are at leaſt told in hiſtory†, that this event gave riſe to an improvement in the apparatus for extinguiſhing fires, and to a new ordinance of the 23d of July of that year reſpecting fires, as well as to the introduction of fire engines with hoſe; which however had been employed at Amſterdam more than a dozen of years before.

Hoſe or pipes of this kind for conveying water were not, however, unknown to the antients. The architect Apollodorus, at leaſt, ſays, in the paſſage already quoted, that to convey water to elevated places expoſed to fiery darts, inſtead of pipes, the bowels of an ox might be employed, having at the one end a bag filled with water, which being compreſſed, the water will be forced through the inteſtines to the place of its deſtination ‡. Theſe were conveyers of the ſimpleſt conſtruction.

Among

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* Breſlauer Samlung 1717. 1 verſuch, p. 108. Paſchii Inventa nov-antiqua, p. 668. W. G. Heſſe Abhandl. zu Verbeſſerung der Feuerſprützen. Gotha 1778. 8vo. p. 7.

† Algemeine Welthiſtorie, th. 33. p. 631.

‡ *Policrcet*, p. 32. Κατα δε τα προκειμενα τοις πυροβολοις μερη, αντι σωληνων, βοων εντερα παραφεροντα ὑδωρ εις ὑψος. Τουτων ασκοι πληρεις ὑδατος παρατιθενται και θλιβομενοι αναφερουσι. In partibus autem quæ expoſitæ ſunt telis incendiariis, pro tubis, boum inteſtina habere oportet, quæ aquam in ſublime deferant.

Among the neweſt methods of improving the hoſe of fire engines, is that of weaving them without a ſeam. In the year 1720 ſome of this kind were made at Leipſic, of hemp, by one Beck, a lace-weaver, as we are told by Leupold in his before-mentioned account of fire engines, printed in that year. After this they were made alſo by Erk, linen-weaver at Weimar; and at a latter period they were made of linen at Dreſden, and alſo in Sileſia [*]. At preſent, Gegner and Ehrliholzer have a manufactory at Bethnal Green [†], where they make water-tight hoſe without ſeams. Aſſeſſor Mögling has cauſed ſome of the ſame kind to be manufactured on his eſtate near Stuttgard on a loom invented by himſelf, and which are now uſed on many of the towns of Wurtemberg [‡]. I ſhall here take occaſion to mention a kind of loom on which Braun cauſed ſhirts without ſeams to be wove, ſimilar to thoſe ſometimes brought from India as curioſities, and of which he gave a particular deſcription, with an engraving [§].

I ſhall in the laſt place obſerve, that notwithſtanding the belief of the Turks in predeſtination, the uſe of fire engines has been lately introduced at Conſtantinople by Ibrahim Effendi [||].

XLI. *Deſcription of a new Electrometer.* *By* HENRY LAWSON, *Eſq.*

SIR, *To Mr. Tilloch.*

AS the following deſcription of an inſtrument, which anſwers its purpoſe very well, may prove acceptable to the public, I ſend it to you for inſertion in the Philoſophical Magazine.

Some time ago it ſtruck me that ſome additions to Brooks's electrometer might be made ſo as to fit it for a good diſcharging electrometer to meaſure the repulſion between two balls

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deferant. Ante hæc inteſtina utres aqua pleni collocantur, qui preſſi aquam ſurſum emittunt.

[*] Leipziger Intelligenzblatt 1775. p 345; and 1767. p. 69; Teutſcher Merkur 1783.

[†] The Environs of London, by Daniel Lyſons; Lond. 1792—1796.

[‡] W. G. Kapp über die Stärke rund gewebter Seile, Tubingen 1795. 8vo. Phyſikal ökonom. Bibliothek xix. p. 258.

[§] Veſtitus Sacerdotum Hebræorum; Amſtel. 1701. 4to. i. p. 273. A great deal of uſeful information reſpecting improvements in the apparatus for extinguiſhing fires may be found in Aug. Niemann *Ueberſicht der Sicherungsmittel gegen Feuerſchäden*, Hamburg und Kiel 1796. 8vo. See *Phyſikal ökonom. Bibliothek* xix. p. 412.

[||] Buſchings Erdbeſchreibung. ii. p. 673.

(of a certain ſize) in grains, and alſo effect the diſcharge of a battery at the ſame time. The inſtrument known by the name of Cuthbertſon's diſcharging electrometer was at that time the beſt, and indeed the only inſtrument for diſcharging batteries or jars by its own action, then made; but I think this will be found in the eſſentials, and in the theory and uſe, a more perfect inſtrument.

On the baſe S (fig. 1. Plate VI.) is fixed the glaſs pillar G, ſupporting the hollow braſs ball B. T, is a light graduated braſs tube divided (from the weight W towards the ball B) into thirty parts, repreſenting grains. W, is a ſliding weight. L, a light braſs ball ſcrewed to the end of the tube T, on the other end of which tube adjuſts the heavy counter balance-ball C, the tube T and its two balls being ſuſpended at their common centre of gravity by a ſilk line in the centre of the ball B, the mechaniſm of which is ſhown in fig. 2. The braſs ball F is ſtationary, and of the ſame ſize as the ball L, and is fixed by a braſs arm to the ball B. The adjuſting ball A is alſo of the ſame ſize, and adjuſts cloſe to the ball L, or at any lower ſtation between that and the ring *r*. The braſs tube to which the ball A is fixed is divided into inches, halves, and quarters: (a more minute diviſion is unneceſſary and improper.) The diviſions begin, or the o line is marked, on the ſaid tube at the ring *r*, when the three balls A L F are cloſe together. The ring *r* ſerves as an index, as the diviſions paſs in ſucceſſion into the glaſs tube P on lowering the ball A. The hook H is ſcrewed into the baſe of P. The quadrant, or Henley's electrometer, Q, is ſupported on a long braſs ſtem to keep it out of the atmoſphere of the lower part of the inſtrument. Fig. 2. ſhows the internal conſtruction of the ball B, fig. 1. In the firſt place, the ball ſcrews in half, horizontally. The light tube T paſſes through the ball, and is ſuſpended nearly in the centre of it by ſome ſilk twiſt *s*, which ſmall ſilk twiſt is fixed into the eye of the adjuſting wire *a*, part of which wire is filed ſquare and goes through the ſquare hole *b*. The nut *n* ſcrews on *a*, and ſerves to adjuſt the light tube T vertically. The light plates P P are of copper, and move freely on the wire *w w* ſomewhat like a hinge, and reſt on the copper wires C C, ſerving to make the direct communication between the inſide and out of the battery or jar. N *N* are notches ſerving to let the tube T deſcend when the diſcharge is made. Into the tube Z the glaſs pillar is ground. Note, that at the bottom of the notch N is a piece of braſs filed with a Y, and ſo placed as to keep the centres of the balls L and F, fig. 1, under each other when they come cloſe together.

When the inſtrument is adjuſted, which is done by placing the weight W, fig. 1, at 0 on the line of grains, and then ſcrewing or unſcrewing the counterbalance ball C, till the tube T riſes ſlowly into its horizontal poſition; then ſet the ball A at the diſtance from the ball L that you chooſe, and the weight W placed at the diviſion or number of grains that you wiſh the repulſive power of the electricity to arrive at before the diſcharge is made: this being done, connect the battery or jar with the ball B by means of the wire *v*, the end of which goes into B at the hole X, and ſhould ſtand at right angles to B, the ball of *v* reſting on the battery: then connect the outſide of the battery or jar with the hook H; as the battery charges, the electrometer Q continues to riſe; and when it is ſo highly charged that the repulſive power between the balls L and F is equal to the number of grains at which the weight W was placed, the ball L will deſcend, and deliver the charge of the battery to the ball A. The ſubſtance or thing through which the ſhock is intended to be paſſed, muſt form part of the communication between the hook H and the outſide of the battery or jar.

It is evident, from conſidering the inſtrument, that it is attended with theſe excellences; that, the whole being compact, the electric atmoſphere muſt be ſo alſo; that it has but one *inſulation* to depend upon; that the equilibrium of the divided arm and ball may always be adjuſted, conſequently the weight uſed known to a certainty; and the hook for communicating with the outſide of the battery being placed on the foot of the inſtrument, the communication may be made by wires *lying* on the table; which are by far the beſt mode of conducting ſhocks, as chains, by the imperfect connection of their links (eſpecially if long), very often, and without being ſuſpected, hinder the ſucceſs of many very well concerted experiments.

XLII. *Deſcription of an improved Chemical Apparatus for preſerving ſeparate the Gaſeous Products evolved in many Proceſſes; by which alſo Abſorption is completely prevented, and Liquids may be ſtrongly impregnated with the different Gaſes.* By W. H. Pepys *jun.* *Eſq.*[*]

THE chemiſt, in his various reſearches into the compoſition and qualities of the numerous ſubſtances preſented by Nature, had long to regret the want of an apparatus capable of preſerving all the gaſeous as well as liquid products evolved from them during his experiments. The apparatus invented by Woulfe, and which will immortalize his name, was the firſt, and indeed may be ſaid to be the only one yet made known, calculated at all to anſwer ſo deſirable an end: but it had defects; and though ſome of them have been removed by ſubſequent improvements, ſtill it has its imperfections. Every chemiſt will readily perceive that I have in my eye the inconvenience experienced on changes of temperature: if abſorption take place between the retort and receiver, a portion of liquid from the firſt Woulfe's bottle ruſhes into the receiver; or, if the tube of Welter is uſed, an undetermined quantity of atmoſpheric air finds admiſſion to ſupply the vacuum thus formed.

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[*] Communicated by the Author.

To obviate theſe inconveniences, in a train of experiments, in which I was engaged in the year 1798, on the oxymuriatic acid and its compounds, after thinking of different contrivances, it ſtruck me that I ſhould be able to accompliſh what I had in view, by procuring an adaptor, mounted with a glaſs valve (ſimilar to that uſed in the improved Nooth's apparatus), and, at the end furniſhed with the valve, ground into a tubulated receiver; and then, by means of a bent tube, connecting the upper opening of the adaptor with a ſeries of Woulfe's bottles. By means of this apparatus, which was executed agreeably to my inſtructions by Mr. Blades, I was enabled to form the pureſt oxymuriates without the ſmalleſt riſque of any accident from abſorption.

By another modification of the apparatus, in which the woulfes were diſpenſed with, I obtained ſome of the fineſt cryſtals of oxymuriate of potaſh I have ever ſeen, without the ſmalleſt portion of gas eſcaping—a circumſtance of no ſmall moment; oxymuriatic gas, as every one knows, being extremely noxious. In this arrangement, two additional adaptors were joined to the one ground into the receiver and furniſhed with the valve as already mentioned; and the uppermoſt adaptor was furniſhed with a tube leading into a pneumatic apparatus; or, when the redundant gas was not to be preſerved, into a chimney. By employing the apparatus in this form, every bubble of gas has to paſs through the three adaptors, and experiences a conſiderable agitation along with the liquor employed in them; the gas forcing the liquid from the loweſt into the ſecond, and from that into the uppermoſt adaptor,—till, a portion of it being taken up by the fluid, and the lower orifices of the upper adaptors being at times left quite free, the liquid, following the common law of gravity, returns to occupy again the lower ſtation. When there is a quick and plentiful ſupply of gas, the whole of the liquid is kept in continual motion, offering thereby a proportionally large ſurface for impregnation, when that is the object in view; as when it is wiſhed, for inſtance, to impregnate water very highly with ammoniacal gas procured from the ſulphate or muriate of ammonia by lime. The quantity of liquid employed at one time in the adaptors ſhould be about ſufficient to fill the loweſt one full, and the middle one about a third part.

The apparatus of which I have been ſpeaking, which is leſs expenſive than ground woulfes, not ſo eaſily injured, and more readily replaced when any part is fractured, I have found ſo extremely uſeful, and applicable to ſo many purpoſes, that I perſuade myſelf ſuch a deſcription of it as will enable any one to get it conſtructed, will be acceptable to many. United with a ſeries of woulfes, it forms one of the completeſt apparatuſes that has yet been deviſed for a great variety of chemical experiments; for it is not propoſed to ſuperſede the woulfes: indeed, when gaſeous products are intended to be paſſed through a variety of liquids, they will, in many caſes, be found indiſpenſably requiſite *. I ſhall take the preſent opportunity, now that we are ſpeaking of Woulfe's apparatus, of mentioning a few circumſtances which, having found uſeful myſelf in conſtructing a ſeries of woulfes, may prove equally ſo to others.

Lavoiſier frequently regretted the want of a perfect Woulfe's apparatus, the uſual method of manufacturing which is by grinding the tubes to fit the bottles after the tubes have been bent: from this circumſtance it generally happens that the bottles have no equal ſtanding, but are obliged to be accommodated to the bend of the tubes and the fitting of the necks by means of wedges and ſupports put under the bottles, which continually expoſes the tubes to the riſque of being broken during the adjuſtment of the parts. To remedy this inconvenience, when I want tubes, I cauſe them to be ground to fit the woulfes while ſtraight (See A, fig. 4. Plate VII.); I then bend them into two angles of about 130° or 140°, juſt above the ground ſtopper parts (See B, fig. 4.): then taking the two bottles to which the tube has been ground, and placing

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* In other caſes, however, even where gaſes are to paſs through different liquids, they could be diſpenſed with, if, as Mr. Chenevix ſuggeſted when I ſhowed him the apparatus, a ſet of adaptors were provided, each furniſhed with a valve, like the loweſt adaptor, inſtead of the bent tubes now attached to them. The gas evolved might by this means be made to paſs through any number of adaptors, which would ſave all the trouble attending glaſs tubes. The valves, however, would conſiderably increaſe the expenſe.

them on a true plane, and heating the tube, as exactly as I can in the centre between theſe angles, I there form a third angle (C, fig. 4.) while the glaſs is ſoft and yielding[*], making it to accommodate itſelf to the ground necks, in which it is left without being moved for about a quarter of an hour to cool thoroughly. If moved too ſoon, the tube is apt to alter its form. In this manner, tubes may be bent, one at a time, for a range of any extent; and when arranged afterwards on any plane ſurface, will always be found to fit.

I ſhall here mention a kind of tubes which I have found extremely convenient on many occaſions: I mean tubes made of elaſtic gum or caoutchouc. They are compoſed of a web or tube of wove cat-gut, coated outſide and inſide with elaſtic-gum varniſh. They ſtand the action of moſt of the gaſes. Ether, alcohol, or oils, however, would deſtroy them. The accommodation they yield is very great; as the woulfes may be agitated or moved in any direction, and may be placed on the moſt uneven ſurfaces. Theſe tubes are fitted into corks previouſly impregnated with wax or tallow; and when they are to be made uſe of, I firſt put a ring of elaſtic-gum (a neck of a gum bottle) over each neck of the woulfe, but ſo as to ſtand a little higher than the glaſs, forming a kind of wall round the cork and tube, to receive melted wax, or lute, to ſecure them in their places.

Deſcription of the improved Apparatus.

Fig. 1. (Plate VII.) A, the retort joined to a tubulated receiver B. C, the adaptor ground into the neck of the tubulated receiver, and furniſhed with a glaſs valve made in the ſame manner as thoſe uſed in the improved Nooth's apparatus. D, woulfes joined to the adaptor to receive the unabſorbed gas: and E, a bent tube to carry the gas that may ſtill paſs unabſorbed either into a pneumatic apparatus or into a chimney.

Fig. 2. the valve on a larger ſcale, and inſerted in the neck of the adaptor. It conſiſts of an internal tube of ſmall calibre but pretty ſtout in ſubſtance, and ground into an exterior tube cloſed at the upper end, but perforated with ſeveral ſmall holes to allow the gas to paſs. After the internal tube is ground to fit the external, a portion of it is cut out (as at *a*) ſufficient to receive a ſmall hemiſphere of glaſs, and to allow the hemiſphere to riſe a little in its ſmall chamber, but not to turn over in it. The upper piece of the internal tube is then thruſt home to the place in which it is to remain, and the glaſs hemiſphere introduced, with its plane incumbent on the upper end of the lower piece of the internal tube, which is ground perfectly flat, as is alſo the plane of the hemiſphere. From the conſtruction, it is obvious that, by the upward preſſure of any gas, the glaſs hemiſphere may be raiſed ſo as to allow the aëriform fluid to paſs into the adaptor, but that there can be no return of any thing into the receiver even when a partial vacuum takes place in it; and the greater the exceſs of the expanſive force or preſſure exerted at any time in the adaptor over that maintained in the retort and receiver, the cloſer does the valve become.

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[*] If the tubes are not very thick, the heat from a flare lamp of tallow and cotton-wick is ſufficient for the purpoſe. When the tubes are thick I prefer a charcoal fire made in a black lead crucible, acroſs which the tubes may be eaſily heated to the required degree. Whichever of theſe methods be followed, the heat ſhould not be applied too ſuddenly; nor ſhould the tubes be blown upon, or expoſed to a low temperature, to cool them haſtily, as by ſuch treatment they are apt to fly.

Fig. 3. two adaptors, A and B, ground to fit into each other, and alſo to fit into C of fig. 1. Any number may be fitted to each other in the ſame manner. By this apparatus liquids may be highly impregnated with the gaſeous products evolved during diſtillations. The bent tube T is for the purpoſe of conveying the ſuperabundant gas to a chimney or to a pneumatic apparatus.

Fig. 4. improvement in the mode of fitting the glaſs tubes in a Woulfe's apparatus. This has already been ſufficiently explained.

XLIII. *Life of* JOSEPH TOALDO, *Profeſſor of Aſtronomy and Meteorology at Padua.*

JOSEPH TOALDO, whoſe merit as a philoſopher and meteorologiſt is well known in all the civilized countries of Europe, was born on the 11th of July 1719 at San Lorenzo di Pianezza, a ſmall village near Maroſtica, at the foot of the Alps, in the valleys of Vicenza. The firſt part of his education he received from various eccleſiaſtics, who gave him an early taſte for the ſciences, and accuſtomed him to diligence and application. In the year 1733 he was ſent to the ſeminary of Padua, where he ſtudied Latin, rhetoric, philoſophy, and theology, but in particular the mathematics. Here he took the degree of doctor of theology, and was deſtined for the place of a teacher in the ſame inſtitution in which he had been inſtructed. About this period he began to diſtinguiſh himſelf as a writer. The firſt literary labour he engaged in was a new edition of the works of Galileo, to which he added various fragments never before publiſhed, together with annotations and a preface. This edition contained alſo the celebrated dialogues reſpecting the ſyſtem of the world. As one of the maſters of the above ſeminary, he taught grammar, rhetoric, philoſophy, and mathematics; and here he firſt introduced the infiniteſimal calculus according to the principles of his preceptor Suzzi, one of the moſt celebrated analyſts of Italy. His ſervices to this eſtabliſhment were ſoon rewarded by the archbiſhop with the benefice of Montegalda, which he enjoyed for fourteen years. Though this place engaged a great deal of his attention, it allowed him ſufficient leiſure for continuing his mathematical ſtudies, which, on account of the different occupations of his academical office, he had not been able to purſue with that attention which he wiſhed. He however exchanged this benefice for another more convenient, after he had been invited by the ſenate of Venice, in the year 1762, to take upon him the vacant profeſſorſhip of aſtronomy and meteorology in the univerſity of Padua.

In this inſtitution he found it neceſſary to make a great many changes in order to render his inſtruction uſeful. The firſt thing to be done, according to the ſyſtem which he propoſed, was to erect an obſervatory. The curators of the univerſity complied with the requeſt which he made on this ſubject, and intruſted to him the care of drawing up the plan and ſuperintending the conſtruction of the building. On this account he undertook a tour through Italy for the purpoſe of inſpecting the principal obſervatories, that he might be better able to deviſe a plan for that of Padua. The foundation of this obſervatory was laid in the year 1767, and in 1774 it was completed. He now procured an excellent quadrant from England, which he employed for making obſervations in conjunction with his nephew and aſſiſtant Chiminello. To ſupply the want of good elementary books he publiſhed a ſhort view of plane and ſpherical trigonometry, with tables, under the title of *Tavole Trigonometriche, con una Introduzione, que contiene un Compendio di Trigonometria piana e ſpherica, applicata alla Pratica, con molte altre Tavole*; Padua 1769. 4to.; which was afterwards reprinted and introduced into many of the Italian ſeminaries.

This work was ſoon followed by another on the influence of the heavenly bodies on the weather and atmoſphere; which greatly contributed to extend his fame as a philoſopher, and which contained the reſult of a long ſeries of meteorological obſervations made by him. The original title is, *Della vera Influenza degli Aſtri nelle Stagioni e Mutazioni di Tempo, Saggio meteorologico ſondato ſopra lunghe Obſervazioni*;

Padua 1770. 4to. This work, which made the author known throughout all Europe, was tranſlated into different languages, and procured for him admiſſion into various learned ſocieties. About the ſame time he publiſhed ſome eſſays in ſavour of electrical conductors, many of which, in conſequence of his recommendation, were erected in the Venetian territories: alſo a chronological view of uncommon changes in the weather, with tables of the ſtate of the barometer, and flux and reflux of the ſea. In the year 1773 he began his meteorological journal, which he continued till his death. In the year 1774 his celebrity was ſtill further increaſed by his anſwering a prize queſtion, propoſed by the Academical Society of Montpellier, on meteorology as applied to agriculture; which was crowned by the above ſociety, and printed in Rozier's *Journal de Phyſique.* It was tranſlated in almoſt every country of Europe, and, together with the author's preceding work, excited general attention to a ſtudy which before had been much neglected: this paper contributed alſo, in a conſiderable degree, to the eſtabliſhment of the Meteorological Society of Manheim. After this period the author continued with great zeal to exert himſelf in diffuſing meteorological knowledge, and in improving meteorological inſtruments; and in 1776 he publiſhed, in the Economical Journal of Venice, an extract from De Luc's Treatiſe on the Barometer and Thermometer, by which the conſtruction of theſe inſtruments was much facilitated. Next year (1777) he tranſlated Lalande's Aſtronomical Tables and his *Abregé de l'Aſtronomie,* as he did afterwards his *Aſtronomie des Dames*; and he erected a marble buſt of that eminent aſtronomer in the obſervatory. After this time his attention was excluſively directed to aſtronomy and meteorology, and he in particular endeavoured to confirm more and more his doctrine reſpecting the influence of the moon on the different changes of the weather. He publiſhed alſo an hiſtorical view of the ſervice rendered by the Venetian ſchools to aſtronomy, geography, and navigation. In the year 1783 he obtained, in conjunction with his nephew Chiminello, from the academy of Manheim, the prize for the beſt treatiſe on the conſtruction of a comparative hygrometer; and in 1784 he publiſhed a ſmall work on finding the longitude, *De Methodo Longitudinum, et Obſervatione Tranſitûs Lunæ per Meridianum*; which was much approved, particularly in England. From this period he wrote nothing except his journal till the year 1787, when he publiſhed a ſmall work in two ſheets, entitled *Confronto delle Ragioni coi principali Prodotti della Campagna,* which appeared at Venice. His Tables of Vitality, *Tavole di Vitalita*, a ſmall but laborious work, was printed next year at Padua and Venice.

In the year 1788 he undertook a third tour * to Rome, Naples, Trieſte, and Tuſcany; during which he examined the place where Hannibal croſſed the Alps, and wrote a paper on this ſubject, which was inſerted in the fourth volume of the Tranſactions of the Academy of Padua, of which he had been a member from the time of its eſtabliſhment. Next year he introduced the French clocks at Padua, and publiſhed a treatiſe on gnomonics, and reſpecting *ſchediaſmata aſtronomica*, the eclipſe of the ſun, and the paſſage of Mercury.

Beſides theſe works, the public were indebted to him for various ſhort eſſays, publiſhed ſometimes in his own journals and printed ſometimes ſeparately: ſuch as a ſhort treatiſe on chronology; an eſſay on the extraordinary winter, together with a chronological view of the weather in general; ſeveral reſearches reſpecting the continued drought of the winter of the year 1779; on fogs, and the influence of fiery meteors; a prognoſtication of the weather from the flight of birds; conſiderations on a new cycle, and the ſtate of the planets; general rules for foretelling the periods of rain and wind in the Adriatic gulph, from an obſervation of the heavens. Other papers by him are to be found in many of the public Journals, and in the Tranſactions of learned ſocieties. The Journal of Modena contains his defence of Leibnitz againſt De Luc, and obſervations on the falling of the mercury in the barometer during rainy weather; that of Piſa, a treatiſe on the influence of the moon, in anſwer to the objections of Friſi. Various meteorological and philoſophical papers written by him may be found likewiſe in the Venetian, Vicentine, and Milaneſe journals, in ſome of the French journals, and in the Tranſactions of the Society of Manheim. The Philoſophical Tranſactions, thoſe of the Inſtitute of Bologna, and of the Academy of Berlin, contain three of his papers, viz. *De Æſtu reciproco Maris Adriatici*; *De Calore Lunari*; and *De Vi Lunæ in Atmoſphæram, ex Obſervationibus barometricis*. Some of his aſtronomical obſervations were publiſhed in the Memoirs of the Academy of Sciences at Paris. The Tranſactions of the Academy of Padua, however, contain the greateſt number of papers written by this diligent philoſopher: ſuch as, a deſcription of an *aurora borealis*, with a chronological account of the appearance of this phænomenon ſince the time of the Romans; a determination of the longitude and latitude of the obſervatory of Padua; a treatiſe on the phyſical properties of the atmoſphere in different places, from thermometrical and barometrical obſervations, and from the phænomena of vegetables; obſervations on thunder; reſearches reſpecting the heat in ſeveral parts of Italy, from the latitude of 41 to 47°; deſcription of a beautiful fire-ball ſeen on the 11th of September 1784 at Padua; and a great many aſtronomical obſervations made in conjunction with Chiminello. He left behind him ſeveral works in manuſcript, among which were, obſervations on the travels of Marco Polo, and on the real epoch of the celebrated Chineſe wall.

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* Beſides the tour above mentioned for inſpecting the Italian obſervatories, he had undertaken a ſecond about the year 1780, through Lombardy, Piedmont, and to the coaſt of Genoa.

Amidſt ſuch occupations Toaldo ſpent his days in tranquillity, notwithſtanding the unhappy ferment which prevailed in his native country; and might perhaps have been ſtill alive had not his end been haſtened by chagrin. A fruitleſs attempt made in favour of a young man who had been deprived of his office, gave him ſo much uneaſineſs, that he firſt loſt his natural vivacity, and ſoon after felt himſelf indiſpoſed; on the 7th of November 1797 he was attacked by a nervous affection, and died on the 11th, in the 79th year of his age.

Toaldo was of ſmall ſtature, had a friendly aſpect, and in general an appearance that inſpired confidence. His conduct was amiable, his deportment eaſy, and his converſation, which was animated, diſplayed great knowledge and an extenſive acquaintance with various branches of ſcience: ſimple in his manners, open and ſincere, he abandoned himſelf only to the ſofter paſſions, and ſeemed to have no other ambition than that of being uſeful. He was ſteady in his friendſhip, always ready to do good offices in the moſt diſintereſted manner, and indulgent towards every one. To the talents of a literary man he added the virtues of the citizen, and therefore was univerſally reſpected, eſpecially by thoſe who enjoyed his intimate acquaintance.

XLIV. *Account of the Diſcovery of Coal at the Cape of Good Hope*[*].

IN moſt families at Cape Town a ſlave is kept expreſsly for collecting fire-wood. He goes out in the morning, aſcends the ſteep mountains of the peninſula, where waggons cannot approach, and returns at night with two ſmall bundles of faggots (the produce of ſix or eight hours hard labour) ſwinging at the two ends of a bamboo carried acroſs the ſhoulder.

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[*] From *Barrow's Travels.*

Some families have two and even three ſlaves, whoſe ſole employment conſiſts in climbing the mountains in ſearch of fuel. The expenſe of a few faggots, whether thus collected or purchaſed by the load, for preparing victuals only, as the kitchen alone has any fire-place, amounts, in a moderate family, to forty or fifty pounds a year.

The addition to the inhabitants of five thouſand troops, and a large fleet ſtationed at the Cape, increaſed the demand for fuel to ſuch a degree, that ſerious apprehenſions were entertained of ſome deficiency ſhortly happening in the ſupply of this neceſſary article. Under this idea, the attention of the Engliſh was directed towards finding out a ſubſtitute for wood. The appearance of all the mountains in ſouthern Africa being particularly favourable to the ſuppoſition that foſſil coal might be found in the bowels of moſt of thoſe inferior hills connected with and interpoſed between them and the ſea, his excellency the earl of Macartney, well knowing how valuable an acquiſition ſuch a diſcovery would prove to the colony, directed a ſearch to be made.

Boring-rods were prepared, and men from the regiments who had laboured in the collieries of England were ſelected to make the experiment. Wynberg, a tongue of land projecting from the Table Mountain, was the ſpot fixed on, and the rods were put down there through hard clay, pipe clay, iron-ſtone, and ſand-ſtone, in ſucceſſive ſtrata, to the depth of twenty-three feet. The operation of boring was then diſcontinued, by a diſcovery then made of actual coal coming out, as miners expreſs it, along the banks of a deep rivulet flowing out of the Tygerberg, a hill that terminates the iſthmus to the eaſtward. The ſtratum of coaly matter appeared to lie nearly horizontal. Immediately above it was pipe clay and white ſand-ſtone, and it reſted on a bed of indurated clay. It ran from ten inches to two feet in thickneſs; different in its nature in different parts; in ſome places were dug out large ligneous blocks, in which the traces of the bark, knots, and grain, were diſtinctly viſible; and in the very middle of theſe were imbedded pieces of iron pyrites, running through them in crooked veins or lying in irregular lumps. Other parts of the ſtratum conſiſted of laminated coal of the nature of turf, ſuch as by naturaliſts would be called *lithanthrax*, and pieces occurred that ſeemed to differ in nothing from that ſpecies known in England by the name of Bovey coal. The ligneous part burned with a clear flame without much ſmell, and left a reſiduum of light white aſhes like thoſe of dried wood. The more compact earthy and ſtoney parts burned leſs clear, gave out a ſulphureous ſmell, and left behind a ſlaty caulk, that ſoon contracted on the ſurface a deep brown ochraceous cruſt. The borer being put down in ſeveral places in hopes of meeting with the main bed of coal, the general reſult was as follows:

In the bed of the rivulet:	Feet.
Coal - - - - -	2
Blue ſoapy rock - - - -	5
White ſoapy rock - - - -	22
Gray ſand-ſtone with clay - - -	21
Sand-ſtone of chocolate brown - -	14
Blueiſh ſoapy clay - - -	31
Striated ſand, red and white, containing clay -	33
	128

Here the operation was diſcontinued.

XLV. *Notices reſpecting New Books.*

BABYLONIAN INSCRIPTIONS.

THOUGH a great many travellers ſpeak of the ruins of the antient Babylon, and deſcribe the bricks of which theſe ruins are compoſed, very few of them have noticed the ſingular inſcriptions or characters impreſſed on them. Pietro della Valle, a moſt circumſtantial and accurate traveller, even carried ſome Babylonian bricks to Italy, one of which he preſented to Kircher, then reſiding at Rome, to be preſerved in his muſæum, ſtill extant in that city; but neither he himſelf, nor that learned jeſuit, who was ſo much engaged in reſearches reſpecting Egyptian, Chineſe, and other kinds of oriental literature, ever made mention of Babylonian inſcriptions.

The firſt by whom they were noticed ſeems to have been father Emanuel, a Carmelite friar, who, having reſided ſome time at Bagdad, in his manuſcript account, ſpeaks of characters impreſſed on the antient bricks ſtill remaining among the ruins of Babylon, which, d'Anville ſays, would ſupply the literati, who are deſirous of penetrating into the remoteſt antiquity, with entirely new matter for meditation and ſtudy.

After him Niebuhr, in his Travels to Arabia, mentions inſcriptions on the Babylonian bricks ſtill extant, but without telling whether they contained characters unknown, or ſimilar to any already diſcovered; only, when ſpeaking o the bricks on which they are inſcribed, he ſays, that he ſaw inſcriptions of the ſame kind on other bricks at Bagdad and in Perſia.

But the moſt circumſtantial, and the moſt recent notice reſpecting them, is that of M. Beauchamp, correſpondent of the Royal Academy of Sciences at Paris, who, by reſiding ſeveral years at Bagdad, had more leiſure to examine and deſcribe the ruins of Babylon; which he did in his account inſerted in the *Journal des Savans* for 1790.

In this account, the author, ſpeaking of the remains of Babylon, ſays,—"On the ſide of the river are thoſe immenſe ruins which have ſerved, and ſtill ſerve, for the building of Helle, an Arabian city containing ten or twelve thouſand inhabitants. Here are found thoſe large and thick bricks, imprinted with unknown characters, ſpecimens of which I have preſented to the abbé Barthelemy." What kind of characters, however, theſe were, neither Beauchamp nor that celebrated antiquary, who wrote with ſo much ability on the Phœnician, Palmyrenian, and other inſcriptions, thought proper to determine; and it is only by his ſucceſſor, M. Millin, that we have lately been made acquainted with the exiſtence of ſome Babylonian bricks at Paris, containing inſcriptions, which were ſent by Beauchamp from Babylon, and of which drawings or copies were tranſmitted to M. Herder, at Weimar, and to profeſſor Munter, at Copenhagen.

In the mean time, the Honourable Eaſt India Company, being always deſirous to lend their aſſiſtance to thoſe who may be employed in the elucidation of oriental antiquities, and being informed that, near the town of Hillah, on the river Euphrates, there exiſt the remains of a very large and magnificent city, ſuppoſed to be Babylon; and that the bricks of which thoſe ruins are compoſed, are remarkable for containing, on an indented ſcroll or label, apparently a diſtich, in characters totally different from any now made uſe of in the Eaſt, directed the governor of Bombay to give orders to their reſident at Baſſorah, to procure from thence ten or a dozen of the bricks, and to tranſmit them, carefully packed up, as early as poſſible to Bombay, that they might be thence forwarded to them in one of their ſhips ſailing for England.

In conſequence we were gratified, at the commencement of the preſent year and century, at London, with the firſt view of inſcriptions, which, on comparing them with the Perſepolitan characters as given by Le Bruyn, Chardin, Niebuhr, and other travellers, appeared to be of the ſame origin, being only more complex, and connected by long lines, forming whole and half ſquares, ſtars, triangles, &c.;

ſo that they prove to be a different combination, though formed of nearly the ſame elements and nail-headed ſtrokes.

It is well known that, for more than a century paſt, about which time the Perſepolitan inſcriptions were firſt diſcovered by European travellers, opinions have been much divided reſpecting theſe characters. Some have believed them to be taliſmans, and others the characters of the Guebres, or antient inhabitants of Perſia; others held them for mere hieroglyphics, and others for alphabetic characters, like ours. Kæmpfer ſuppoſed them to expreſs whole ideas, like the Chineſe characters, but that they had been appropriated ſolely for the palace of Iſtakhar.

After that period, however, ſome of a ſimilar kind were found alſo in Egypt; but as neither the Egyptian hieroglyphics, nor the characters obſerved on the mummies, had the leaſt reſemblance to them, they ſerved only to prove the connexion, which we know from hiſtory that Perſepolis once had with Egypt. Raſpe, on finding ſome others on a cylinder of loadſtone, perſuaded himſelf that they were the ſame with the Chineſe characters; and, conſequently, that the Chineſe writing had been formerly known and cultivated on this ſide of the Ganges.

The above account of the diſcovery of theſe inſcriptions is taken from Dr. Hager's preface to his late work entitled *Diſſertation on the newly diſcovered Babylonian Inſcriptions*[*], in which the learned author endeavours to prove that the Perſepolitan characters were derived from the Babylonian.

"By the Babylonian bricks here exhibited," ſays the learned author, "the whole difficulty in regard to their origin is removed; as it is evident that Babylon, in point of cultivation, was much earlier than Perſepolis, and that the Chaldeans were a celebrated people when the name of the Perſians was ſcarcely known.

"To confirm this opinion, and by it to prove that the Perſepolitan characters were derived from the Babylonian, I have thought it neceſſary to begin this work by a brief examination of the antiquity, extent, and ſciences of the Babylonians; and through ſcantineſs of original monuments, to prove, by aſtronomy, architecture, and languages, their well-founded claim to antiquity. At the ſame time I have endeavoured to ſhow that not only the Perſians, but alſo the Indians, were diſciples of the Chaldeans; and that the Egyptians themſelves, who pretend to be the inſtructors of all nations, probably derived their pyramids and obeliſks from Babylon. Proceeding then to the Babylonian inſcriptions, I have ſhown their ſimilarity to that celebrated alphabet which the Indians call divine or celeſtial (*deva-nagari*), becauſe they believe that it was communicated by the Deity himſelf in a voice from heaven; and I have tried to prove that they were not derived from heaven, but from our earth, and from the borders of the Euphrates. I have confirmed my aſſertion by means of the Tibetan characters, thoſe acknowledged deſcendants of the Indian ones, and thus endeavoured to invalidate the opinion of that great antiquity and boaſted originality of the Bramins."

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[*] To be had at Richardſons, and Cuthell and Martin's.

In a future Number we ſhall give a further extract from this curious work: in the mean time we preſent our readers with a ſpecimen of the inſcriptions. (See Plate VIII.)

MEDICINE.

Profeſſor Schumacher, of Copenhagen, has publiſhed the firſt volume of his Medico-chirurgical Obſervations. As the author attends an hoſpital, he had occaſion to make curious remarks worthy of confidence. His zeal induced him to ſubſtitute in his practice the uſe of indigenous for that of foreign plants. He has employed the *gratiola* inſtead of ipecacuanha. In ſuch caſes when this plant does not produce its effect, he recommends aſſiſting it with the addition of a little rhubarb. He has diſcovered that, in wounds and ulcers attended with inflammation, the *cortex hippocaſtani* or *cortex ſalicis* produces the ſame effect as *cortex peruvianus*, which is far dearer. The *carex arenaria*, alſo, may be ſubſtituted for ſarſaparilla. It reſults from the author's experiments, that the *fabæ pichurim* are an efficacious remedy for the whites; eſpecially when the diſeaſe is inveterate, and ariſes from weakneſs.

Dr. Tode, of Copenhagen, continues alſo, with ſucceſs, his Medico-chirurgical Dictionary. In the firſt number of the fifth volume the author informs the faculty that a fœtus of 199 days is ſuſceptible of being brought into the world alive, but without all the ſymptoms of perfect conformation, and without being in a ſtate to prolong its exiſtence.

MEDICAL JURISPRUDENCE.

Citizen Bellock, a French phyſician, has lately publiſhed a work entitled *Legal and Judicial Medicine.* The object of this work is to inquire how far medical knowledge may be uſefully employed in legal inveſtigations. The author has treated the ſubject with much ability, and with all that cautious *pyrrhoniſm* ſo neceſſary in an inquiry of ſuch importance. Among the points he diſcuſſes we ſhall notice the following:—1. Are there criteria by which we may diſcover whether a perſon has hanged himſelf, or been hanged by others?—2. Can it be aſcertained whether a human body found dead in water has been drowned, or whether the perſon had been thrown in after death?

He does not omit to notice the two famous trials of the laſt century, in which much light was thrown on theſe two queſtions. The one related to the unfortunate Calas condemned at Toulouſe, and ſolemnly juſtified at Paris: the other to young Sirvan, found drowned in a well a league from his father's houſe.

BOTANY.

Dr. Schrader, one of the moſt celebrated German botaniſts, publiſhes at Göttingen a botanical journal containing an account of the moſt important diſcoveries in regard to the ſcience of vegetables. This journal, written in Latin and in German, appears every three months. Each number or volume conſiſts of about 450 pages duodecimo, illuſtrated with three plates, each of which exhibits ſeveral figures. The author divides each number into four parts. The firſt is deſtined for the papers and memoirs addreſſed to him; in the ſecond he gives an extract from new works; the third contains the moſt important diſcoveries and obſervations made in the ſcience of botany; and the fourth, every thing moſt intereſting in the correſpondence which the author maintains with the literati in Europe.

Though every part of botany is cultivated with the moſt brilliant ſucceſs in the north of Europe, the ſtudy of the cryptogamia claſs of plants, that is to ſay, thoſe the ſexual organs of which are difficult to be diſcovered, ſeems to engage, in a particular manner, the botaniſts of thoſe countries. To them we are much indebted for the light which has been thrown on this department of botany. The labours of Hedwig, Hoffman, Schrader, Humboldt, Perſoon, Flügge, Stromeyer, &c. have diſſipated the obſcurity with which the nature and origin of muſhrooms were ſurrounded. This journal, therefore, of which ſix numbers have already appeared, as it contains all the new diſcoveries and obſervations in regard to botany, cannot fail to be of utility to thoſe who are fond of this ſcience.

A very valuable work on this ſubject is now publiſhing in Paris. It is an account of new and rare plants collected by C. Cells, with fine plates. The hiſtory of theſe plants is drawn up with great ability by the celebrated botaniſt Ventenat,

tenat, of the National Inſtitute. Four numbers of this work have already appeared: the fifth is about to be publiſhed.

MINERALOGY.

A very valuable treatiſe on this ſubject has juſt been publiſhed by C. Haüy, member of the National Inſtitute, and keeper of the mineralogical collections of the School of Mines; 4 vols. 8vo., and a quarto volume of plates. A fine edition of the ſame work, conſiſting of the ſame number of volumes, in 4to., was publiſhed at the ſame time.

XLVI. *Proceedings of Learned and Economical Societies.*

ROYAL SOCIETY OF LONDON.

AT the anniverſary meeting on the 30th of November the amount of receipts and expenditures was declared, and the names of the new and the deceaſed members were read over by the ſecretary.

The preſident, the Right Hon. Sir Joſeph Banks, K. B. in the name of the Society, preſented Sir Godfrey Copley's gold medal to Mr. Aſtley Cooper for his paper on the effects which take place from the deſtruction of the membrana tympani, and on an operation for the removal of a particular ſpecies of deafneſs.

Afterwards the Society proceeded to the choice of the council and officers for the enſuing year.

The members, as uſual, on that day dined together at the Crown and Anchor tavern, in the Strand.

Dec. 10. was read a paper by Dr. Maſkelyne, aſtronomer royal, drawn up from the notices publiſhed by Piazzi, of his diſcovery of a new planet rather larger than our earth, and revolving in an orbit between Jupiter and Mars; to which he propoſes giving the name *Ceres Ferdinandia.* All doubts reſpecting this diſcovery will be reſolved at its next appearance in March 1802.

On the 17th was read a paper, by Everard Home, Eſq. giving an account of one of the moſt extraordinary ſubjects of comparative anatomy that has ever appeared—a deſcription of the *Platypus anatinus*, an amphibious animal, of which a male and a female were brought from New South Wales. They are there found on the banks of one of the lakes, in which they ſwim not on the ſurface, but occaſionally come up to reſpire. It has four ſhort legs furniſhed with claws, and the mouth is terminated by a bill like a duck. The parts of generation are moſt uncommon, and, from the female not having a uterus, Mr. Home is led to ſuſpect it is oviparous.

THE JABLONOWSKY SOCIETY OF THE SCIENCES AT LEIPSIC.

This Society has propoſed for 1802 the following

Prize Queſtions:

Hiſtory.—An account of the rights and condition of the dignity of earl in the northern kingdoms.

Mathematics.—An accurate view of the progreſs and improvement of the mechanical ſciences during the eighteenth century.

Economy.—On the influence of the atmoſphere on the fertility of the ſoil, according to the lateſt experiments and reſearches. How far may the nature, ſituation, and culture of the ſoil contribute to render this influence active and powerful?

The facts adduced by the candidates in their papers muſt be confirmed by the teſtimony of reſpectable authors; but the papers themſelves, according to a condition eſtabliſhed by the founder, muſt be written in Latin or in French. The prizes conſiſt of gold medallions of the value of 24 ducats each.

The ſociety invites all the friends and promoters of ſcience to anſwer the above queſtions. The anſwers muſt be tranſmitted, with a ſealed note containing the name and reſidence of the author, to Charles Frederick Hindenburg, profeſſor of philoſophy at Leipſic. The period for receiving the anſwers has not yet been announced.

ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

In the ſitting of May 2, M. Reſch laid before the ſociety ſeveral ſpecimens of leather tanned, according to his proceſs, by means of one-fourth bark and three-fourths turf (black peat-moſs), which were found exceedingly good: on account, however, of ſome doubts excited in regard to the activity of turf, he means to cauſe his experiments to be repeated.

M. Homeyer, of Zimmer near Alfeld, tranſmitted to the ſociety ſome twigs grown together, in order to ſhow how mankind may have fallen upon the invention of grafting.

Two papers were read, one on the *aſplenium* and ſome kinds of fern related to it, by M. Bernhardi, of Erfurt; and experiments on the preparation of cinnabar in the wet way, by M. Bucholz, of Erfurt.

In the ſitting of June 2, profeſſor Frank read a paper on the queſtion lately propoſed by Meſſrs. Anton and Kreſchman—whether the origin of Bards and the Druids can be juſtly aſcribed to the old Germans? The author is of the opinion of thoſe who anſwer this queſtion in the negative.

FRENCH NATIONAL INSTITUTE.

Account of the labours of the French National Inſtitute during the laſt three months of the year 9.

[Continued from p. 188.]

Obſervations made during three Years on the metallic Chains ſuſpended from the Dome of the Pantheon to aſcertain any horizontal or vertical Change that might take place in the Building. By C. Prony.

The late miniſter of the interior, Benezech, having appointed a commiſſion to reſume the labours of thoſe before eſtabliſhed for examining the Pantheon, and pointing out the means that might be neceſſary for its preſervation, C. Prony, member of this commiſſion, read an intereſting memoir on this ſubject, which was heard with great attention. Five metallic chains, compoſed of links each 3·937 inches in length, are ſuſpended below the cupola of the Pantheon. Four of theſe chains are parallel to the vertical ſurfaces of the planes of the pillars; the fifth is attached to the centre of the exterior dome, and hangs down near to the ground. The centres of five horizontal circular plates are made to correſpond very exactly to the points where the weights with which each chain is loaded terminate. Theſe plates are raiſed on vertical graduated ſtalks, which riſe and fall freely in ſockets, and which can be fixed to any height by means of ſcrews. Concentric circles are traced out on each plate, and meteorological inſtruments placed in different parts of the edifice indicate the differences of temperature.

Chains ſuſpended in this manner can ſink down only in conſequence of three cauſes. 1ſt, The dilatation of the metal, which would augment their volume and the length of their maſs: 2d, Elongation, occaſioned by an extenſion which a certain weight might give to that length where the ſyſtem is interrupted by links: 3. The partial or total ſinking of the edifice.

The effects which changes of temperature might produce on a metallic ſyſtem, interrupted by links, were examined with as much accuracy as it is poſſible to attain to in reſearches of this nature, with an inſtrument invented by that eminent artiſt Ramſden. Lavoiſier and Laplace repeated

theſe

theſe experiments at the houſe of C. Prony, and ſhowed that plates of different metals are lengthened by a given and conſtant quantity, from the temperature of ice to that of boiling water *.

The ſecond cauſe of elongation is eſtimated by a method deſcribed with great accuracy in Prony's memoir.

The third was that of the greateſt importance to be known to the commiſſion; for ſome architects entertained fears that the edifice was ſinking. This movement, according to the hypotheſes of ſome, by cauſing the pillars, which are ſaid to be too weak to ſuſtain the maſs, to give way, would have cruſhed them, and the dome would have ſunk vertically on itſelf.

The poſſibility of ſuch a diſaſtrous effect ſuggeſted to the commiſſion a deſire to aſcertain how far this was probable. Prony has detailed, in his memoir, a great number of precautions which he took to eſtimate this danger. After a long and accurate inveſtigation, he was convinced that no alteration has taken place in the ſurfaces of the walls of the foundation, and particularly in the vertical direction of the pillars; that the paralleliſm of the circular cornices of the cupola is ſtill preſerved; that the three domes are in no manner affected; and that an examination of the pillar which has ſunk moſt, proves it to be ſound. The bliſtering or ſcaling off of the compoſition which ſurrounds the pillars was occaſioned therefore only by the ſinking of the latter, and the ſinking aroſe from an error in the conſtruction. But can this ſinking make any progreſs? The attention of the commiſſion will no doubt be directed to this object; and the inſtrument invented by C. Prony is calculated to aſcertain it.

New Galvanic Experiments.

It appears that the identity of galvaniſm and electricity is perfectly eſtabliſhed by recent facts. C. Biot has found that the galvanic fluid in the different kinds of apparatus which produce it, follows exactly the ſame progreſs as electricity would under the like circumſtances.

Attraction and repulſion, indeed, being the ſame in galvaniſm as in electricity, the action which the moleculæ exerciſe upon each other is the ſame in the two fluids; the property which points poſſeſs of tranſmitting electricity, and which ſurfaces have to retain it, ought alſo to take place in galvaniſm; and, as the ſmall plates may be conſidered as more analogous to points than the large ones, they ought to give in the ſame inſtant a leſs maſs of fluid, but poſſeſſing a great velocity, while the large plates will give a greater maſs with a leſs velocity. Shocks which depend on the velocity of the fluid will not be increaſed with the ſize of the plates; while metallic combuſtions which depend on its maſs and its continued preſence, ought with this ſize to be doubly augmented. This is a ſimple explanation of the experiments of C. Hachette, Fourcroy, Vauquelin, and Thenard.

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* Theſe elongations are eſtimated at an 864th for ſteel, and a 900th for caſt iron: the dilatation of forged and hammered iron differs very little from that of ſteel.

C. Biot has conſidered galvaniſm alſo in regard to its chemical nature; and finding that what takes place in water brought into communication with the pile has been ſufficiently examined, he was deſirous of ſeeing what takes place in the pile itſelf, and what its action is on the ſurrounding bodies.

As it is well known that the zinc of the pile ſoon becomes oxidated when it acts, it was neceſſary to diſcover whence the oxygen aroſe. For this purpoſe C. Biot, in company with C. F. Cuvier, made experiments, which proved that the atmoſpheric air is decompoſed, and gives up its oxygen to the pile; that this oxygen increaſes the galvanic effects, but that it is not abſolutely neceſſary to them; that theſe effects take place even *in vacuo*, provided the pile is well moiſtened with water; which proves that the water is decompoſed.

Guyton found that the abſorption of the oxygen of the air by the pile is ſo complete, that it may be employed as an eudiometric mean to meaſure the quantity of oxygen contained in any given kind of air.

Biot next employed this oxidation of the pile as a means of confirming his theory reſpecting the progreſs of the fluid. In the caſe when the fluid poſſeſſes a greater velocity the oxidation ought to be accelerated; ſmall plates therefore become ſooner oxidated than large ones; a pile the two ends of which communicate by wires is alſo oxidated much ſooner, as might be expected, than that where there is no ſuch communication.

The ſame author has diſcovered that the galvanic fluid is tranſmitted with difficulty through water, but that it eaſily glides over its ſurface; an effect produced alſo, according to Prieſtley's experiments, by weak electricity poſſeſſing great velocity.

C. Ganthenet, who is not a member of the Inſtitute, has communicated ſome intereſting experiments on the ſame ſubject, in which he ſhows, in particular, that it is poſſible to make effective galvanic piles without any metallic ſubſtance whatever. Theſe experiments ſerve to confirm thoſe before made by Pfaff and Humboldt when they tried galvaniſm only on animals.

While theſe reſearches were making in France, the Engliſh made ſome of a peculiar and deciſive kind. They have been able to imitate the moſt ſingular effects of galvaniſm by common electricity, by making the conductors much thinner and longer.

CHEMICAL ARTS.

New Method of bleaching Linen.

This method, invented by Seguin, conſiſts in employing one part of ſulphuric acid in five hundred parts of water.

New Method of bleaching the Pulp of which Paper is made.

The art of bleaching raw cloth almoſt immediately, by means of the oxygenated muriatic acid, is the invention of Berthollet. Chaptal made a happy application of it to reſtore the whiteneſs of books and prints that have been ſtained; but it remained to employ it for previouſly bleaching the pulp of which paper is made. This was attempted by Loyſel ſome years ago, in order to render it difficult to imitate aſſignats, on account of the perfection of their manufacture. This paper money, in a political view, may be conſidered ſeveral ways; but as an object of art, it is certain that it has occaſioned a multitude of ingenious inventions, which will long remain uſeful after aſſignats ſhall have been forgotten. The different proceſſes are now gradually made public, ſince they are no longer ſecrets of ſtate. The public are already in full poſſeſſion of the ſtereotype art; and that of the fabrication of paper, no leſs intereſting, is now laid open alſo. It is attended with the double advantage of beauty and œconomy.

By means of this method, all rags may produce a pulp exceedingly white; but they are not all proper for making paper ſtrong, and difficult to be broken, though thin,—a quality eſſential to bank paper, and others of the ſame kind. New white or raw rags, cordage, and other articles, the hemp and flax of which have not been much worn, ought to be choſen for this purpoſe. Old rags do well enough for thick paper, or that which does not require much tenacity.

When the rags have been picked and cut into ſmall morſels, they are put into vats, where they undergo a fermentation, which changes the nature of their colouring matter, by rendering it, as it were, ſaponaceous, and ſuſceptible of being carried off by water when waſhed, and afterwards beat. The heat, during the fermentation, muſt not be ſuffered to exceed 100° or 112° of Fahrenheit's thermometer, which generally takes place in fifteen days.

The moment of ſubjecting the rags to the action of the acid is not a matter of indifference. If they are bleached entire, as the interior of each thread does not experience any action, this would afterwards alter the whiteneſs of the paſte; and if the workman ſhould wait till they were abſolutely reduced to pulp, the pulp would form a body with which it would be difficult to mix the acid intimately. The matter then muſt be taken at a mean ſtate between that of rags and that of pulp completely triturated. For this purpoſe the rags are made to paſs under a firſt cylinder to ſeparate the fibres; they are then ſubjected to bleaching, and then converted into pulp by means of another cylinder.

If the rags are raw, two baths of oxygenated muriatic acid or bleaching liquor, and one of ſulphuric acid, are employed. If they are white, one bath of the liquor and one of ſulphuric acid will be ſufficient.

The muriatic acid is prepared either with common muriatic acid and oxide of manganeſe, or by means of ſulphuric acid with oxide of manganeſe and muriate of ſoda (common ſalt). The laſt method is the moſt economical; 25 parts of oxide of manganeſe, 70 of common ſalt, and 120 of ſulphuric acid, of 31 degrees of denſity (by the areometer), will be ſufficient to charge a receiver of 270 gallons. The ſtrength of this liquor is appreciated by means of a ſolution of indigo, which is prepared by diſſolving one part in weight of indigo in ſeven parts of ſulphuric acid concentrated to 66 degrees, and which is then diluted in 992 parts of water, according to the method of Deſcrozilles. When one meaſure (in volume) of the bleaching liquor annihilates the colour of nine of this ſolution of indigo, the bath proper for bleaching 110 pounds avoirdupoiſe of rags, after being ſubjected to the firſt cylinder, is compoſed of 24 gallons of ſuch bleaching liquor diluted in 120 gallons of pure water.

The expenſe of theſe different operations will be moderate, when one bath of bleaching liquor and one of ſulphuric acid are employed, which is generally the caſe, and the paper is exceedingly beautiful. There is no doubt therefore that the price of paper will be much diminiſhed, when the method here explained is employed and brought into general practice.

On the Fixity which Antimony acquires by its Alloy with Tin.

When attempts were made to reſtore to commerce that prodigious quantity of metallic matters which had been employed in making bells, it was found that the firſt operation neceſſary was to ſeparate the copper and tin, which formed their principal conſtituent parts. The copper was eaſily obtained by calcining a part of the bell metal, and then throwing it upon another part of the ſame metal in a ſtate of fuſion; but the ſcoriæ produced, during this reduction, were loſt.

More intelligent chemiſts, and particularly Anfry, found means to reduce the ſcoriæ, and to reſtore to commerce a large quantity of tin, and a ſtill larger of copper. The tin, however, obtained in the laſt reſult has peculiar qualities; its fracture is different from that of common tin, it is harder, and more eaſily broken.

C. Sage has found means to imitate this tin, extracted from bell-metal, by mixing with tin a little antimony; and it reſults from his experiments, that the latter metal alloyed with tin, in the proportion of a ſixth part, unites with it in ſo intimate a manner that it is almoſt impoſſible to ſeparate it. C. Sage has no doubt then that the tin of bell-metal contains a certain quantity of it; but C. Sage has aſcertained, by ſeveral experiments, that it does not prevent it from being employed in making tin plate, and even for tinning veſſels.

MINERALOGY.

On the preſumed Effects of Galvaniſm in the Mineral Kingdom.

C. Guyton has given a deſcription of a kind of ore of antimony lately diſcovered in the province of Gallicia, ſpecimens of which had been tranſmitted to him by Don Antonio Angulo, inſpector general of the mines of Andaluſia. He ſoon found, by analyſis, that the metal in this ore is in the ſtate of oxide; but the ſtructure of the mineral, its interior texture, its well characterized ſtriæ, plainly announced that it was the paſſage from ſulphuret to oxide, without an alteration of form. It was therefore of importance to diſcover how this change could have been effected.

C. Guyton tried all the ſimple or compound ſubſtances which might be preſumed to exiſt in the bowels of the earth, in order to imitate, in the laboratory, this labour of nature. The ſulphuret of antimony, both natural and artificial, reſiſted all his numerous attempts. He ſuppoſed, very juſtly, that the decompoſition of water ought to act a principal part in this operation, and even that it preſented the only means of accounting for this alteration, ſince it ought at the ſame time to furniſh oxygen, and to take away the ſulphur by means of the hydrogen; but it was neceſſary to diſcover what could have determined this decompoſition. C. Guyton conſiders it as the ſlow and progreſſive reſult of affinities put into action by the galvanic fluid. A compariſon of what takes place in experiments where metals are ſpontaneouſly oxidated, by the interpoſition of water, appears to him to give great probability to this explanation, which he ſtrengthens by ſeveral examples of ſimilar transformations, particularly that of the pyrites of Berezof, which paſſes to the ſtate of oxide, retaining, in three directions, the ſtriæ of the primitive ſulphuret. He quotes alſo numerous obſervations, by which C. Haüy has proved the influence of ſubterranean electricity on minerals. He entertains no doubt that this new idea will extend the field of mineralogy, by expoſing to us nature in continual labour, acting at the ſame time on maſſes and on intimate moleculæ, by ſpontaneous attractions, and independent of all percuſſion; ſo that this ſubtle fluid, according to Guyton, will aſſume the firſt rank among thoſe ſubſtances which have been dignified with the pompous name of *mineralizers.*

Analyſis of a phoſphated Lead Ore.

This ore, of a yellowiſh green colour, generally in ſmall brilliant grains, and in a quartzy arenaceous reddiſh matrix, is found in pretty thick continued beds at Breitemberg, near Erlembach, in the department of the Lower Rhine. C. Sage has found, by an analyſis, that it contains, in 100 parts, 37 of lead, 33 of quartz, and 30 of phoſphoric acid. The lead contains no ſilver.

Emeralds found in France.

This diſcovery was made by Lelievre. Travelling near Limoges, he obſerved, in the ſtones employed for repairing the roads, a hard greeniſh ſubſtance, which he judged to be a beryl or an emerald. His conjecture was confirmed by the chemical analyſis which Vauquelin made of ſpecimens ſent by him to that chemiſt, and by Haüy's examination of their mechanical ſtructure. Theſe emeralds will not be of much uſe as ornaments; but they may be employed by chemiſts to extract from them that kind of earth called *glucine*, which Vauquelin diſcovered in the emerald and the beryl, and which theſe emeralds of Limoges readily furniſh, as C. Lelievre found ſeveral pounds of them, and was even able, by combining the fragments, to recompoſe a priſm of more than four inches in diameter, and nearly the ſame in height.

METEOROLOGY.

On the Action which the Lunar Period of Nineteen Years exerciſes on the Variations of the Atmoſphere.

As a decided influence on the variations of the atmoſphere has always been aſcribed to the moon, ſeveral methods have been tried to determine the nature of this influence. Some have thought that it was exerciſed by means of the phaſes: others have ſuppoſed that it depended on the reſpective poſition of the moon and the ſun; and as this poſition recurs nearly the ſame at the end of nineteen years, the latter have concluded that meteors muſt alſo return almoſt the ſame in the courſe of that time. C. Lamarck, who has ſuppoſed another influence of the moon in regard to its declination, and who has endeavoured to eſtabliſh his diſcovery by ſeveral memoirs, has alſo endeavoured to aſcertain what truth there may be in this periodical return of the variations of the atmoſphere at the end of nineteen years; and he has found, by comparing meteorological obſervations, that this return is far from being ſo correct as is generally believed. Aſtronomers alſo know well, that the cycle of nineteen years is not exact within an hour and a half; an error which amounts to a whole day in the courſe of 308 years.

ZOOLOGY.

On the Difference which exiſts between the Crocodiles of the old and the new Continent.

A preciſe determination of the large ſpecies of animals is more important than might be imagined, as we have inſtances that, for want of ſuch determinations, travellers have placed in America ſeveral animals of the old continent which never exiſted there, ſuch as the lion, tiger, panther, and ſome others. Theſe they confounded with other animals of a different kind, though nearly related to them. The ſame error has occurred in regard to crocodiles, even on the part of naturaliſts. In order to rectify ſuch miſtakes, C. Cuvier has ſhown, that moſt authors have been entirely unacquainted with the difference which exiſts between the crocodiles of the old and the new world, or have badly underſtood them, and explained them ſtill worſe.

He has proved, by an accurate deſcription, that theſe animals form two ſpecies, the diſtinguiſhing characters of which are as follow:

1ſt. The *crocodile* has an oblong muzzle, the upper jaw of which has indentations on each ſide to afford a paſſage to the fourth tooth in the lower; its hind feet are entirely webbed.

2d. The *caiman* has an obtuſe muzzle; its upper jaw receives the fourth tooth of the lower in a particular cavity which conceals it; the hind feet are only half webbed.

The firſt of theſe ſpecies belongs to the old world, the ſecond to the new. The name of the latter originates from

India, where it denotes the common crocodile, and from which it muſt have been introduced into America by the Spaniards or the Dutch.

The author has not comprehended in his reſearches the *long beaked crocodile*, or the *gavial*, which forms, by general acknowledgment, a peculiar ſpecies.

On two new Kinds of oviparous Quadrupeds.

Theſe two ſpecies, deſcribed by Lacepede, are intereſting not only on account of their novelty; they are intereſting alſo as they exhibit a number of toes not before obſerved but in the claſs of reptiles.

The firſt, which Lacepede calls *monodactylos*, has indeed only one toe on each foot. Theſe feet are ſo ſhort, and the body and tail ſo long, that the animal has a great reſemblance to a ſnake; it is covered with ſcales diſpoſed in tranſverſal bands.

The other ſpecies, called *tetradactylos*, has feet as ſhort, and a body as long, as the preceding; but each foot is furniſhed with four toes, and the body is marked on each ſide with a longitudinal furrow.

Theſe two ſpecies will form hereafter two new genera in Lacepede's genera of *lizards*.

On a new Kind of Inſects called Atractoceros.

This inſect was brought from the kingdom Owara, in Africa, by Paliſot-Beauvois. The name given to it by that traveller ſignifies the *ſpindle horn*, and indeed ſuch is the form of its antennæ. It is diſtinguiſhed from other coleoptera by its wings being much longer than the cover, and not folding under them, and by having five articulations in all the tarſi. This laſt character brings it near to the *ſtaphilini*, while the preceding gives it an analogy to the *necydales*. The form of its antennulæ is very ſingular. C. Beauvois thinks that this inſect lives in the woods.

ANIMAL PHYSIOLOGY.

On the Quantity of Air neceſſary for the Reſpiration of a certain Number of Individuals in a Space where it is not renewed.

Experiments made with machines proper for navigating under water, made laſt year at Havre, and lately by the Engliſh engineer Folkeſtone, made Guyton conceive the idea of comparing, on that ſubject, the conſequences of the chemical theory of reſpiration, the reſults of obſervations made under the diving-bell, and the attempts made for the ſame purpoſe by Drebell and Halley. After having calculated, hour by hour, the conſumption of the reſpirable part of the air, and the formation of carbonic acid gas, he indicates the chemical means for ameliorating what remains of the former, and of abſorbing the latter; and, in the laſt place, of preventing the danger ariſing from accumulated animal emanations, by the momentaneous opening a bottle of oxygenated muriatic acid, which appears to him very proper to be ſubſtituted in the place of the liquor mentioned by Boyle as having ſerved to cool the air in a veſſel rowed under water.

PHILOSOPHY OF VEGETABLES.

On the Influence which the Air and other aëriform Fluids have on Germination.

This new labour of Sennebier is a ſupplement to his large work on the phyſiology of vegetables. The experiments were made by placing the ſeeds on moiſt flannel, under bells filled with different kinds of gas, the influence of which it was intended to try. All theſe ſeeds refuſed to germinate in azote, in carbonic acid, and in pure hydrogen gas. The preſence of oxygen was in general neceſſary, and its employment is in part to abſorb the carbon of the ſeeds, to form of it carbonic acid. What is ſingular is, that pure oxygen is not the gas moſt favourable to germination; it accelerates germination, but it weakens it. The action of oxygen muſt be meliorated by the preſence of a ſubſtance inactive by itſelf; and we here find that wonderful fact preſented to us by ſo many other circumſtances of the organic economy, viz. that the mixture moſt favourable to germination is that of which the atmoſphere is formed, that is, about three-fourths of azote to a fourth of oxygen; and germination does not take place at all if there is not an eighth of oxygen at leaſt in the atmoſphere.

But the manner in which oxygen is introduced into this atmoſphere is not a matter of indifference; it muſt be poured into it ſuddenly. Were it made to enter only little by little, the firſt portions would ſcarcely be ſufficient to abſorb the carbon of the ſeeds, and no more would remain to vivify them.

Seeds may be made to germinate alſo, by mixing oxygen with carbonic acid, or with hydrogen. In the latter caſe, the carbon which iſſues from the ſeeds unites itſelf intimately with the hydrogen. Too much carbonic acid in air does more hurt to ſeeds than too much azote, and too much azote more than too much hydrogen. Various vapours may alſo alter the air, ſo as to annihilate germination: of this kind are thoſe of ſulphuric ether, of camphor, of oil of turpentine, of vinegar, of ammonia, &c.

This neceſſity of oxygen in nature is liable, however, to ſome exceptions. Some ſeeds have ſuch force, that they decompoſe the water to ſeparate from it oxygen, if they cannot diſpenſe with it entirely: of this kind are peas, which germinate in water deprived of air, in every ſort of gas, and even in oil, provided they have been before ſteeped in water.

AGRICULTURE.

Progreſs of the Amelioration of Wool.

Teſſier and Huzard gave an account to the claſs of the ſale of the wool and ſheep of the flock of Rambouillet in the year 9, as well as of the progreſs of the amelioration of wool in France during that period. The moſt intereſting experiment mentioned by them was, that of leaving the wool on ſome ſheep for two years: by theſe means it acquired double length, gave a double weight, without any inconvenience to the animals, and was rendered exceedingly proper for the manufacture of woollen ſtuffs, ſo that it was employed in making kerſeymeers, which were preſented to the claſs, and which are equal to the beſt Engliſh ſtuffs of that kind.

PHILOMATIC SOCIETY OF PARIS.

C. Sylveſtre read a note on the long wool of the ſheep of Rambouillet. It was long believed that ſheep loſe their wool every year; and this aſſertion, void of foundation, has been advanced in works of great reſpectability. The members of the Council of Agriculture, charged with the inſpection of the national ſheep eſtabliſhment at Rambouillet, were deſirous of verifying this fact; and for that purpoſe ſuffered the ſheep to remain two or three years without being ſhorn. By theſe means they obtained very long wool of uniform fineneſs, and which was equal in weight to what would have been produced by two or three ſhearings.

C. Vauquelin communicated a note on the nature of the earth eaten by the inhabitants of New Caledonia. Humboldt, in one of his letters to C. Fourcroy, ſpeaks of an earth eaten by the Otomaquas when they experience a ſcarcity of proviſions*. C. Labillardiere has confirmed, by an obſervation made in a part of the world very diſtant from that inhabited by the Otomaquas, a fact equally ſingular—where the inhabitants of New Caledonia, when urged by hunger, ate large quantities of a greeniſh ſteatites, tender and friable. It may now be readily conceived how the horrid cuſtom of eating priſoners of war might be introduced among a ſavage people reduced to ſuch a ſtate of famine as to be obliged to ſuſpend their hunger by diſtending their ſtomachs and inteſtines with an earthy ſubſtance which has no other alimentary quality than that of being friable.

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* For this letter ſee Philoſophical Magazine, vol. X.

Vauquelin, being deſirous of aſcertaining the nature of this earth, and whether it contained any thing nutritive, analyſed, by the known means, ſpecimens of it tranſmitted to him by C. Labillardiere.

This earth is ſoft to the touch, and conſiſts of ſmall fibres eaſy to be divided: it becomes red in the fire, and loſes $\frac{\text{[illegible]}}{100}$ of its weight. It contains 37 parts of pure magneſia, 36 of ſilex, 17 of oxide, 3 or 4 of water, and 2 or 3 of lime and copper.

It contains therefore no nutritive part, and can be conſidered only as a burthen—as a mechanical mean for ſuſpending the pain occaſioned by hunger.

The ſame chemiſt read a notice on blue oxide of iron. This ſubſtance, ſent to the Council of Mines by baron De Molt, is of a bright blue colour: it preſents itſelf under the form of ſmall inſulated maſſes in the cavities or fiſſures of quartz and hard greeniſh ſteatites. It is friable, but a little unctuous to the touch. It loſes its colour by the flame of the blow-pipe, and then fuſes into a greeniſh white glaſs.

It loſes its colour neither by acids nor by weak alkalies; a circumſtance which diſtinguiſhes it from *lapis lazuli* and pruſſiate of iron. It communicates to the muriatic acid in which it has been put to digeſt, a ſaffron yellow colour, and loſes a little of its own colour; but it cannot be entirely deprived of its colour without diſſolving it at the ſame time: nothing then remains but the ſame quantity of ſilex, which ſeems to ſerve it as matrix.

In examining the muriatic acid which ſerved for this operation, it is ſeen that it has diſſolved alumine, lime, and oxide of iron; but neither mangaueſe, ſulphurated hydrogen, nor phoſphoric acid, ſubſtances to which ſome might be inclined to aſcribe the blue colour of this oxide of iron, are to be diſcovered in it. The cauſe of the very remarkable colour of this oxide, a colour which it has hitherto been impoſſible to communicate to iron by any chemical means, ſtill remains, therefore, to be determined. It appears, however, that the iron in this oxide is carried to a degree of oxygenation near to a maximum.

Vauquelin read alſo a note on the ſaline ſubſtance called *muriciate of Salzbourg*. This matter, to which Haüy has given the name of *gypſiferous muriate of ſoda*, was alſo ſent to the Council of Mines by baron De Molt. Vauquelin found in it, as Klaproth had done, an union of ſulphate of lime with muriate of ſoda; which gives to the former the property of cryſtallizing in the cubical form: but he remarked likewiſe, that 100 grains of this ſubſtance, broken into ſmall fragments, and expoſed to the moſt violent heat for half an hour, loſt none of their weight: they only became ſomewhat opake.

It is very ſingular to ſee a cryſtallized ſalt entirely deprived of its water of cryſtallization, though compoſed of two other ſalts which generally contain a pretty large quantity of it.

C. Gillet read a note on the emeralds lately diſcovered in France by C. Lelievre. Of the ſubſtances thought to be foreign to the ſoil of France, the emerald is not the only one which has been diſcovered within a few years in that country. C. Gillet mentioned the following mineral ſubſtances found there a little before the revolution: arragonite, anataſe, koupholite, ſtilbite, dypire, minilite ſilex, phoſphated lead, native antimony, carburated iron (plumbago).

Since that period, notwithſtanding the ſmall number of mineralogical tours undertaken, the following have been diſcovered: dolomie, porphyroidal rock with a calcareous baſe, ſulphated ſtrontian, aventurine quartz, anthracite, ferruginous ſcheelin (wolfram), oxidated titanium, oxidated antimony, chromated iron, oxidated uranium, arſeniated lead, &c.

MEDICAL SOCIETY OF BOURDEAUX.

This Society has offered a prize of 300 francs for the beſt account of the doctrine of Hippocrates, or of the Hippocratic ſchool of medicine.

The anſwers to this queſtion will be received till the 30th of July 1802.

COLLEGE OF PHARMACY AT PARIS.

On the 10th of November the College held a public ſitting for the diſtribution of prizes, the miniſter for the interior being preſident.

Bouillon-Lagrange, profeſſor of chemiſtry, after proclaiming the names of thoſe who had obtained prizes, gave an account of the labours of the Free Society of Pharmacy at Paris, and announced the prize queſtion founded by the præfect of the department of the Seine, which is to be determined in the beginning of the year 12. It is as follows:

To determine, by exact experiments, what happens to thoſe ſalts moſt frequently employed, and particularly the ſulphates of ſoda and magneſia, the tartrites of potaſh and ſoda, hyper-oxygenated muriate of mercury, and tartrites of potaſh and antimony, when mixed with the uſual beverages, ſuch as ptiſans, apozems, decoctions, ſoups, whey, the juice of herbs, and potions.

The following memoirs were then read:—Obſervations on the internal temperature of vegetables compared with that of the atmoſphere, by C. Solomé; a memoir on medical wines, by Parmentier; a memoir on phoſphoric ether, by Boudet; obſervations on the formation of a new French pharmacopœia, by Champſeru; obſervations on the juice and ſyrup of gooſeberries, by Bourriat; a diſcourſe on the utility of pharmacy, by Delunel; an obſervation on the exiſtence of phoſphorus in ſugar, by Boullay.

SOCIETY OF AGRICULTURE OF THE DEPARTMENT OF THE SEINE.

This Society has offered a prize of 500 francs for the beſt anſwer to the following queſtion:

1ſt, How does manure act in general? 2d, What are the different kinds of manure ſuited to the different kinds of ſoil, and the different nature of roots and plants? 3d, What are their relative qualities in theſe different caſes? 4th, What are the different ways of preparing theſe kinds of manure? 5th, What are the beſt methods of applying them?

The anſwers to theſe queſtions muſt be ſupported by facts. The prize will be decreed in the month of September 1802. The memoirs muſt be addreſſed, poſt paid, to the ſecretary of the ſociety before the 20th of July the ſame year.

The Society propoſe to open a ſubſcription for the purpoſe of procuring from Spain 4000 of the fine-woolled Spaniſh ſheep, which, according to the treaty of Bale, ſtill remain at the diſpoſal of the French government.

THE ROYAL ACADEMY OF MEDICAL PRACTICE AT BARCELONA.

A prize of 375 rials has been offered by this Academy for the beſt eſſay on the advantages of the cow-pock inoculation; with this expreſs condition, that the facts adduced muſt be ſupported by actual obſervations made by the author.

The prize will not be adjudged till the expiration of four years.

XLVII. *Intelligence and Miſcellaneous Articles.* *December* 1801.

GALVANISM.

ON this ſubject we have witneſſed ſome extremely curious and intereſting reſults ſince our laſt Number. Our preſent notice of them, however, ſhall be brief, as the projected experiments have not all been gone through with thoſe variations of circumſtances which are intended to accompany them. Suffice it therefore to ſtate at this time,

That, in concert with ſeveral gentlemen, zealous promoters of ſcience, and whoſe names ſhall be ſtated hereafter, a pile was conſtructed conſiſting of thirty-ſix pairs of plates of ſilver and zinc, with diſcs of flannel moiſtened with a ſolution of muriate of ammonia interpoſed between the pairs; that each plate was ten inches in diameter, or contained 78·54 ſquare inches; and that, conſequently, the whole ſurface of ſilver in the pile, reckoning only one ſide of each plate, was 2827·44 ſquare inches, and that of zinc the ſame.

With this pile, at a meeting of a committee on the 19th inſt. (December), gold, ſilver, copper, tin, lead, and zinc, were deflagrated with aſtoniſhing facility.

Gold burnt with a very vivid white light, inclining a little to blue. This experiment afforded an opportunity of deciding a point left undetermined in one recorded in our laſt Number, by which gold was deflagrated without any reſiduum being perceived. On the preſent occaſion, there was left on the upper plate a copious oxide of a deep brown colour, inclining a little to purple. In the former experiment, the end-plate of the trough to which the gold was applied was vertical; the oxide could be but little in quantity, and, being nearly of the colour of the mahogany on which it fell, accounts ſufficiently for its not having been perceived.

Silver gave a vivid green-coloured flame, extremely brilliant. The colour was ſomewhat like that of a pale emerald, and the light more intenſe than that from the gold. Gave an oxide inclining to black.

Copper preſented phænomena ſimilar to thoſe obſerved in deflagrating gold.

Lead gave a light of a dilute blueiſh purple, very vivid.

Tin, a light ſimilar to that of the gold; but it burnt with leſs energy,—perhaps owing to the leaves being thicker.

Zinc gave a blueiſh white flame, fringed at the moment of contact with red. It was more difficult to be deflagrated than any of the former, but the plates at the ſame time were much thicker.

The oxides of theſe laſt four metals were not examined.

Some water having been poured upon the upper plate, ſo as to form a kind of little ſtanding pool, ſeveral of the metals were preſented to the plate through this water, and were deflagrated, giving the ſame coloured flame as when preſented to the bare plate. A vapour was ſometimes perceivable inſtantly after the deflagration, ſuppoſed to be a portion of the water converted into ſteam by the intenſe heat.

One circumſtance is extremely worthy of remark. The ſhocks from this pile, though in a ſtate to produce ſuch powerful effects on the metals, could be taken with very little inconvenience.

Dr. Van Marum and profeſſor Pfaff, of Keil, have lately been engaged in a ſeries of experiments with the Voltaïc pile, by means of which they are ſtated to have charged an electrical battery of 140 ſquare feet of ſurface by mere contact with the pile. The account that has reached us does not ſtate whether the battery received as full a charge as could have been communicated by an electrical machine, or only ſuch a one as to become ſenſible to an electrometer. If the latter, the quantity of ſurface named tends to miſlead, for a ſingle jar might have given the ſame indication; but if 140 ſquare feet received a full charge, the experiment will prove a moſt intereſting one. The ſame account ſtates, that, " by the *electricity* of this apparatus, (namely, the Voltaïc pile,) they fuſed 12 inches of iron wire: even platina wire was fuſed by it." If we underſtand the language here uſed, it means that the wire was fuſed, not through the medium of an electrical battery charged by means of the pile, though the introduction of the word *electricity* may ſeem, at firſt view, to convey that idea, but by being brought directly into the Voltaïc circuit. If we are right in this opinion, it was, properly ſpeaking, a deflagration and not a mere fuſion of the wire that took place; *i. e.* the metal was wholly, or in a great meaſure, oxided, inſtead of being altered in form only, as is the caſe in ſimple fuſion: if ſo, it was an experiment of the ſame nature as thoſe related above, in which ſeveral ſquare inches of gold and ſilver leaf, &c. were deflagrated. The detail of Dr. Van Marum and profeſſor Pfaff's experiments, which theſe gentlemen have promiſed to publiſh in a ſhort time, will, however, ſoon clear up the ambiguities of the ſhort account that has reached us.

FULMINATING MERCURY.

The effects of an accidental exploſion of a quantity of the fulminating mercury of Mr. Howard, which was experienced at Dr. Pearſon's laboratory, while at lecture, a few days ago, ſeem to be ſufficiently remarkable to be worthy of being recorded, and may poſſibly throw ſome light on the agency of this powder, and indicate ſome uſeful applications in practice.

The powder juſt collected from the filtering paper, on which it lodged after the liquid from which it was precipitated had paſſed through, was put on paper, and, for expedition, laid in four looſe parcels, (each of which, when dry, would have weighed about thirty grains,) on a tile one inch and a half thick and eight inches ſquare, which had been previouſly heated to a degree a little above what could be borne by the hand. The tile was placed on a table, and left without further notice, to allow the powder to become dry.

At the end of about half an hour the whole of it exploded, making a report equal at leaſt to that of the diſcharge of a 32 pounder, as declared by a naval ſurgeon then preſent. The tile was ſhivered into a number of pieces, and broken quite through the middle; two panes of a window, at the diſtance of about eight inches from the tile, were alſo ſhivered; one of which was oppoſite, and the other immediately above it; but none of the adjoining ſquares of glaſs were broken. On another ſide, at the diſtance of about two feet from the tile, ſtood a frame containing bottles of ſpecimens of chemical preparations, not one of which was broken; nor did any of the pupils ſuffer, who were ſitting on a form only three feet from the powder which exploded.—The whole of the room was filled with thick white fumes; and the windows were covered with quickſilver, depoſited from the exploded powder.

1. The above accident ſhows that the mercurial preparation cannot be employed for throwing projectiles like gunpowder, the mode of agency of the exploſion being on quite different principles, as is evident from what is above related; particularly in the fracturing of the thick tile, and the very little diſtance to which the concuſſion of the air was propagated.

2. It appears that the fulminating mercury is calculated for ſhivering rocks or large ſtones by its exploſion.

3. From the comparatively low temperature at which the powder exploded, it may be employed for certain deſtructive purpoſes in time of war, by being confined in veſſels that are liable to be heated.

4. From the almoſt infinite velocity with which a train of this powder may be fired, it is calculated for ſetting on fire things which cannot be effected by a train of gunpowder *.

ANTIQUITY OF THE EARTH.

In a former Number we mentioned the French having found in Egypt, on ſome of the antient monuments, different figures of the zodiac, which, ſuppoſing them to have been originally formed when the heavens had a correſponding poſi-

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* Communicated by Mrs. R. Lowry, a pupil of Dr. Pearſon.—For a further account of this powder, we beg to refer our readers to Mr. Howard's paper, given in a preceding volume.

tion, tended to give to our earth a much higher origin than our received chronology attributes to it. According to ſome philoſophers, we muſt go back from the preſent period 15,000 years; and even then we ſhall only arrive at the time in which the figures of the zodiac were contrived: but the world may have undergone many previous revolutions.

With all the ſemblance of ſagacity and truth which accompanies this mode of argument, nothing can be more unphiloſophical than the inference. Is it not an admitted fact, that one of the firſt efforts of aſtronomers, after their ſcience had attained a certain degree of perfection, was to draw from it ſuch modes of arrangement and calculation as might be uſeful in adjuſting the *current* time; and that the period from which they *ſuppoſed* their account to ſet out was in a great meaſure *arbitrary*, depending on the multiplication of cycles into each other, counting backward from any poſition of the heavenly bodies in the then current age, to the period in which theſe bodies (according to their tables) muſt have ſtood in a poſition fitting the purpoſe they had in view?

Does not the evidence adduced in different papers in the Aſiatic Reſearches amount almoſt to a demonſtration that, excepting a period which accords very well with the Scripture accounts, the whole Bramin chronology of *myriads* of years is merely a clumſy aſtronomical fiction, which at firſt was not even intended to deceive?

We may alſo ſtate here another well known fact,—that it was long a favourite opinion with aſtrologers, that all the planets muſt have been in conjunction* in the firſt ſcruple of the ſign Aries when the world was made; becauſe then, according to them, was the proper time to begin a year! Others might have been for a different arrangement, *i. e.* a differently conſtructed zodiac; but whichever had the greateſt number of ſuffrages at any period, can never prove any thing as to the fact in queſtion.

But, not to dwell on a ſubject ſo unprofitable, or on the early propenſity of different nations to *forge* for themſelves an high antiquity, is it not known to every one that the Julian period had an arbitrary commencement? If the hiſtory of this fact had been loſt, and correſponding zodiacs had been found, would it have been correct to have inferred that they were made 6500 years ago?

LITERATURE.

Lord Elgin, our ambaſſador at Conſtantinople, having ſent from that city to Athens ſeveral artiſts, who cauſed diligent ſearch to be made in the Temple of Theſeus, we learn that they were ſo fortunate as to find there almoſt all the bas-reliefs which formerly ornamented the Temple of Minerva. Theſe monuments, a great many inſcriptions, and all the urns capable of being tranſported, have been ſent by the Porte to England.

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* Let the planets be all without any latitude in ſuch a conjunction—when did it take place?

A M. de Hammer alſo has made valuable diſcoveries at Paphos, and has found at Roſetta a complete copy of the original of the Arabian Nights Entertainments. Indeed, ſeveral copies of this work, in Arabic, have been found in Egypt, as well by Mr. Hammer as by Mr. Clarke of Jeſus College, Cambridge, and by the members of the French Inſtitute; but Mr. Hammer's attainments are infinitely more extenſive, and excluſively embrace ſome objects of the higheſt importance to the literary world.

Mr. Hammer is a German gentleman of rank, who was ſent by the Emperor of Germany, for the purpoſe of exploring the literary treaſures of Perſia, Arabia, Egypt, &c. During the eventful, and we may ſay miraculous, campaign of Sir S. Smyth in Syria, Mr. Hammer, then on his travels, became known to him, and profited by the extenſive influence and character of our countryman to proſecute his reſearches. He has by theſe means, ſeconded by extenſive talents, and a perfect knowledge of the Arabic and other languages, obtained more intereſting matter than even the French Inſtitute was able to collect. Amongſt other curioſities, he has brought to this country an Arabian manuſcript of great antiquity, furniſhing a complete key to the hieroglyphics, by which he has been enabled to tranſlate various inſcriptions on the pyramids, &c. and thus eſtabliſh the objects for which thoſe ſtupendous maſſes were erected.

Le Moniteur (the French official journal) of the 18th December, announces that the Chevalier de Coral, the Spaniſh ambaſſador at Conſtantinople, received intelligence at Hermanſtadt, on his journey, that a learned Spaniard, now travelling at the king's expenſe, has diſcovered, in Morocco, a complete manuſcript copy of Titus Livius, written in Arabic.

DEATHS.

Lately, at Chateauneuf, in the department of Saone and Loire, in the houſe of his brother-in-law, C. Drée, the celebrated Dolomieu, member of the National Inſtitute, and well known by his important labours in regard to mineralogy.

On the 19th of November laſt, at Nice, Joſeph de Beauchamp, the celebrated aſtronomer, who reſided many years at Bagdad and other places in the Eaſt.

XLVIII. *Remarks upon ſeveral Experiments made to prove the Converſion of Iron into Steel by means of the Diamond.* By DAVID MUSHET, *Eſq. of the Calder Iron Works**.

IN the Philoſophical Magazine for November 1799 are inſerted ſeveral experiments performed by me to prove whether the experiment of the French chemiſts at the Polytechnic School relative to the formation of ſteel by means of the diamond was ſufficiently concluſive. Reſting upon the reſult of theſe experiments, no inference could be drawn to validate the accuracy of the Pariſian experiment. In every caſe, fuſed metallic maſſes were obtained, when neither diamond nor carbonaceous matter was uſed, which, from deſcription, reſembled every way the button obtained by fuſion in contact with the diamond. The reſults produced in my experiments, when hammered, and put to the uſual teſts of heating, and plunging into water, exhibited the uſual ſtate of ſteel, cloſed partially in the grain like ſoft ſteel, and reſiſted, in a conſiderable degree, the application of the file. This was proof beyond that afforded by the French chemiſts. The unuſual teſts of poliſhing upon the lapidary's wheel, dropping the nitrous acid, and exhibiting the fracture of the metallic button, are not ſatisfactory proof to the artiſt that ſteel was here produced.

Some time after the communication of the above experiments, my countryman Sir George M'Kenzie read a paper before the Royal Society of Edinburgh; in the former part of which he exhibited reſults to prove that the experiment of the French chemiſts relative to the diamond forming ſteel was quite concluſive; and, in the latter part of his paper, he detailed a variety of experiments to invalidate a concluſion which I had drawn, of carbon diſſolved in caloric penetrating cloſe veſſels, and by its union with iron forming ſteel. I ſhall in this paper take the liberty of making a few remarks upon theſe different experiments, as they occur in the order of Sir George's arrangement.

I think it proper here to premiſe, that my not ſubſcribing implicitly to the concluſions drawn by the Pariſian chemiſts and Sir George M'Kenzie relative to the diamond and iron, proceeds not from faſtidious ſcepticiſm as to the carbonaceous matter of the diamond; I only wiſh that the fact of its forming

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* Communicated by the Author.

ing ſteel by its union with iron ſhould be eſtabliſhed by experiments more perfect and more rigorous than any hitherto performed, before a concluſion ſo important be drawn and reſted upon.

The remarks made in the firſt paragraph of this paper apply equally to the reſults of Sir George as deſcribed by him, and that of the Polytechnic School. So far as the deſcription of parties, and the teſts uſed by each, go, the reſults obtained in the experiments communicated by me ſeem to prove, with equal certainty, the preſence of ſteel; and, as a further proof of this, I ſhall adduce from the baronet's own experiments an identity of reſults obtained in two of his own operations, wherein it appears difficult to diſtinguiſh betwixt the nature of the products. I allude to that obtained when the diamond was incloſed in a cylinder of iron, and to another experiment, wherein, following the proceſs of Clouet, he fuſed a mixture of iron, carbonate of lime, and pounded crucible *. In the treatment of theſe products reſpectively, no fixed or determinate feature of ſteel is applicable to the one more than the other. If, therefore, in the courſe of ſubſequent communications, I prove, by the moſt unexceptionable experiments, that the decompoſition of the carbonic acid in lime never afforded one particle of carbon to the iron fuſed in contact with it, which iron poſſeſſed ſimilar properties with that obtained along with the diamond, ſo far as the baronet's obſervation and teſts went, will not the experiment of the diamond forming ſteel appear ſtill more equivocal, and require more demonſtrative proofs of its combination with iron than any yet adduced?

Upon the baronet's ſecond experiment, wherein he expoſed a portion of iron under circumſtances ſimilar to thoſe of the diamond experiment, and obtained no fuſion, I ſhall ſhortly remark: That the uncertain and unequal degree of heat obtained in a ſmith's forge, particularly if ſingle blaſted, will eaſily account for a few globules being fuſed in the one caſe, and none in the other. Bliſters were found in both, and it does not appear by any comparative trials of quality that theſe bliſters were eſſentially different, or produced by different cauſes. To prove that an irregularity of heat might produce the difference noticed, namely, the fuſion of a few globules of the iron, a reference might be made to others of Sir George's own experiments, where in one he fuſed iron filings, but could not reduce fragments of horſe-nails!

The major part of Sir George's experiments which follow

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* See Nicholſon's Chemical Journal, No. xl. pages 105 and 107.

thoſe relative to the diamond, divide themſelves into two parts: thoſe wherein heat enough was employed, and in which, conſequently, the iron was found reduced; and *vice verſa.* In every caſe, had the baronet poſſeſſed powers of furnace ſufficient, he would have obtained a fuſed reſult. In thoſe experiments where fuſion was obtained, the quantities were ſo ſmall that they afforded but a ſcanty portion for ſubſequent teſts.

So far, however, we are agreed, that malleable iron is fuſible either alone in a crucible, or in contact with earths of various kinds. It is relative to the nature of the products thus obtained that we differ: a fair ſtatement of the facts, as they relate to the experiments of Sir George, the French chemiſts, and thoſe communicated by me, will beſt explain the points at iſſue.

C. Clouet firſt announced two new proceſſes for the fuſion of iron, and for the converſion of it, in the one caſe into caſt ſteel equal in quality to the Engliſh marks of Hindſman and Marſhall; and in the other, the converſion of it into a kind of caſt iron by means of glaſs. The former experiment, being deemed of national importance, was formally ſubjected to the ableſt chemiſts in France for ſatisfactory documents as to the correctneſs of C. Clouet's pretenſions. This was undertaken and executed by citizens Guyton, d'Arcet, and Vauquelin; who, after repeated experiments in the fabrication and ſubſequent forging of the ſteel, pronounced it, in conjunction with their moſt eminent artiſts, poſſeſſed of all the "external characters and intrinſic qualities of the Engliſh caſt ſteel of the manufactories of Huntzman and Marſchall; that it may be uſed for the ſame purpoſes, and be introduced in competition with it in commerce, without fear of any diſtinction being made to its prejudice."

Shortly after theſe experiments were announced in this country, I reſolved to repeat them along with others, not only with a view to prove the facts eſtabliſhed by the French chemiſts, but more particularly with a view to explain ſome curious reſults in the manufacture of crude iron, the ſource of which I had not before an opportunity to explore, and which would not admit of a ſatisfactory explanation upon Clouet's ſuppoſition of the decompoſition of the carbonic acid and the union of its carbon with iron.

The progreſs and detail of theſe experiments ſhall hereafter be given; ſuffice it at preſent to ſay, that, in following the proceſs laid down by C. Clouet, with clay, carbonate of lime, and iron, a reſult ſimilar to his was obtained: but I found alſo, that when lime, carefully deprived of its carbonic acid, was introduced as a ſubſtitute for the carbonate, the fuſion and the quality of the button produced were in every reſpect the ſame! The metal, ſubjected to a variety of teſts, exhibited the various phænomena mentioned in a former part of this paper. Convinced that, from whatever cauſe this appearance of ſteel was derived, the carbonic acid was entirely neutral, I next ventured to ſubject iron to fuſion *per ſe*; and the reſult anſwered the novelty of my expectations. Beautiful buttons were obtained, of various magnitudes, poſſeſſed of ſimilar properties with thoſe obtained with the carbonate and clay, and with the de-acidified lime and clay. The quality of theſe unalloyed buttons varied, and exhibited, in a greater or leſs degree, the properties aſcribed to the others. From frequent repetitions of the ſame experiment with various ſized crucibles, I found the iron leſs altered in a crucible whoſe capacity was little more than the contents introduced, than when a larger crucible was uſed to the ſame weight of iron.

Thus circumſtanced, and unable to account for this apparent formation of ſteel in any other way, I concluded that ſuch was the affinity of iron for carbon, that when protected from common air, and expoſed to a high temperature, it decompoſed the ignited gas of the furnace, ſeized its carbon, and conſtituted ſteel; and that the degree of ſaturation depended upon the area preſented by the interior ſurface of the crucible to any given quantity of iron.

I had advanced thus far when I communicated the general reſult of theſe experiments in the Philoſophical Magazine for November 1799.

Soon after this, Sir George M'Kenzie read the paper formerly alluded to. In it he informs us that, when he produced heat enough, malleable iron was fuſible *per ſe*; but he alleges that in no caſe was the quality of the metal altered.

In the preſent ſtate of my information upon the ſubject at iſſue, I ſhall cheerfully give up the concluſion I had formerly drawn, of ſteel being formed by carbon diſſolved in caloric. I have ſtated the progreſs of my mind ſo far as it was biaſſed by the experiments formerly communicated; and, far from wiſhing to confound by any arguments advanced in favour of the deduction I formerly made, I regret the facility with which the mind is naturally inclined to indulge in favour of received opinions.

The long received opinion, that pure malleable iron was infuſible, and the unqualified approbation paſſed upon a modification of iron exactly ſimilar in quality, by the ableſt chemiſts in Paris, led me into a falſe appreciation of the quality of the products obtained in my own manipulations. A ſeries of experiments, which I ſhall communicate through the medium of this publication, will, I hope, carry with them the moſt evident demonſtration, that in attributing to their reſults the merits of caſt ſteel, or ſteel of any quality, the French chemiſts have deceived themſelves, and led into error many whoſe confidence in their labours was entire.

I cannot, however, ſubſcribe with the ſame facility to the concluſion drawn by Sir George, that the fuſion of malleable iron does not in the leaſt alter its quality: but I ſhall at preſent wave adducing any experiments to prove the contrary; they ſhall, however, be given hereafter in their proper place. The baronet's experiments afford one inſtance, however, which he has overlooked in forming the opinions on which his aſſertions are founded. He fuſed two parcels of iron filings, the one alone, the other mixed with oxide. From theſe were obtained two buttons of iron exactly ſimilar in quality, which quality, from the eulogium of the artiſt, we may juſtly infer was improved by the fuſion.

The queſtion which moſt forcibly occurs here is this: What revived the oxide in contact with the iron? What affinity, in this caſe, was exerted to diſengage the oxygen and revive the metallic particles? The preſence of carbonaceous matter muſt be inferred. This again we muſt ſuppoſe united to the iron, involving this conſequence, that good malleable iron contains a portion of charcoal capable of reviving at leaſt 1-10th part of its weight of oxide of iron. If this mixture is neceſſary to conſtitute good malleable iron, and a ſubſtance be added, which, to a phyſical certainty, muſt deprive it of this neceſſary conſtituent part, will the fuſed aggregate ſtill be of the ſame quality as a portion of the ſame iron fuſed alone, or an original portion compared with it from the bar*?

I ſhall conclude this paper with the following obſervation, founded upon dear-bought experience, and which I hope neither Sir George nor his artiſt will conceive as in the leaſt degree perſonal.

In ſubmitting metallic reſults to undergo the neceſſary teſts by a workman, the greateſt poſſible degree of caution is neceſſary not to impreſs him with the nature of the wiſhed-for reſult. A ſingle experiment often is the foundation of a theory; and workmen, from the laudable motive of pleaſing a ſuperior, as well as of exciting a diſpoſition to a more valuable remuneration in ſubſequent trials, ſoon diſcover the bias of the operator's mind, and artfully ſtrengthen that predisposing partiality which ſets mature inveſtigation aſide. Thus frequently may a diſintereſted perſon be the innocent cauſe of much error, and the occaſion of many tireſome and laborious experiments.

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* I hope hereafter to ſhow that all malleable iron contains a portion of charcoal; that this is eſſentially neceſſary to its proper exiſtence; and that depriving it of this carbonaceous mixture debaſes its quality by a total annihilation of its properties.

XLIX. *The ſame Methods of Reaſoning common to Mathematics and to Natural Hiſtory and other Branches of Phyſics.* *By Mr.* R. Heron.

SIR, *To Mr. Tilloch.*

YOUR valuable Publication ſupplies much agreeable evidence of the conſtant advancement of phyſical ſcience in all its branches. Yet one cannot help obſerving, that there exiſt, between the votaries of the ſtudies of pure and mixed mathematics, and the ſtudents of chemiſtry and natural hiſtory, a mutual jealouſy and miſunderſtanding, which are, I fear, ſometimes not a little inauſpicious to the general progreſs of philoſophy. You ſhall rarely ſee mathematics and natural hiſtory flouriſh at the ſame time, and in the ſame ſchool of ſcience. The mathematician looks with contempt on the naturaliſt; and that contempt the naturaliſt ſeldom fails abundantly to repay. There is even ſuppoſed to be an incompatibility between theſe different ſtudies, ſo great, that he who excels in the one muſt not hope to ſucceed in the other.

But theſe, Sir, are merely groundleſs prejudices. It is not to propoſe a paradox—when I affirm, that "the truths of mathematics, and the reaſonings by which theſe are diſcovered, have the moſt exact analogy with thoſe of natural hiſtory, and indeed of every other branch of ſcience that can be named." I am induced to ſend you the following ſhort explanation of this propoſition, not in the idea of ſupplying in it a new diſcovery for the uſe of the ſcientific and the learned, whoſe views are truly profound and enlarged; but, as believing it of great importance, that this principle ſhould be rendered familiar to the minds of thoſe who are now entering upon philoſophical ſtudy; and as hoping that ſome of thoſe who are mere bigots of mathematics, and of thoſe who are, on the other hand, mere bigots of natural hiſtory, may be brought mutually into good humour by it.

I. Conſider the nature of thoſe reaſonings by which mathematical truths are diſcovered. Number and figure, points and lines in all their poſſible combinations, magnitudes or quantities, are the ſubjects of mathematical inveſtigation. Of theſe, we bring not into the world, by reminiſcence from a former ſtate, or by innate inſpiration, any original knowledge prior to experience by the ſenſes. So ſimple, however, and primary are their ideas, that they may be conſidered as nearly coeval with the firſt opening of the mind to perception. The line, the point indeed, which we perceive by ſenſe, poſſeſs not each that perfection in its kind which mathematical ſcience aſcribes to it. Yet from ſenſe are our primary notions of the mathematical line and point nevertheleſs derived. We obtain the notions of animal and vegetable forms and colours more perfectly beautiful than any which are actually to be ſeen, ſolely by abſtracting and again combining in idea the beauties which are in nature diſtributed among different animals and vegetables. We form in imagination the character of a truly wiſe and good man by a ſimilar abſtraction and combination of qualities, which in real life are never to be ſeen together. We exalt our minds to conceive the exiſtence of the all perfect Ruler of the univerſe, alſo by the abſtraction of the beſt qualities which we diſcover in his creatures, and by combining them in our thoughts in the higheſt perfection in which we are able to imagine their coexiſtence. And it is juſt in the ſame manner that, by abſtraction from the point and line, which can be materially repreſented to our eyes, we attain to the conception of a mathematical point without length, breadth, or thickneſs; and of a mathematical line, which of theſe qualities has none attributed to it but length.

Thus far, then, the fundamental notions in mathematical ſcience are gained preciſely in the ſame manner in which we arrive at the knowledge of generic, ſpecific, or claſſific characters in natural hiſtory; by obſerving individuals in material nature, and by abſtracting thoſe qualities which are common to numbers; leaving out thoſe which ſerve but to particularize individuals, and making the qualities abſtracted and combined anew, to our minds, the common repreſentatives of all.

Numbers are generated from points; all other definite magnitudes from lines; and a line itſelf is alſo produced from a point. But we have no reaſon to believe, that, if material nature did not ſuggeſt to our conception the repetitions of unity, by preſenting more individual objects than one to our notice, we ſhould ever have formed the idea of two. Nor is it certain that we ſhould have been able, in imagination, to diſtinguiſh lines into the ſpecies of curved and ſtraight, unleſs we had ſeen imperfect examples of both in the extremities or outlines of material bodies. Thus, not only our ſimpleſt mathematical ideas of unity and of extenſion are abſtractions from material exiſtence, juſt as exactly as are the genera, ſpecies, &c. in natural hiſtory; but our notions of the combining of unities and extenſions, by multiplication and addition, ſo as to produce new varieties of them, are certainly in their origin produced by a farther obſervation of material nature, and without this, moſt probably, could not ariſe. The mathematician who purſues, in arithmetic, unity through all its combinations, or who combines, in extenſion, lines into all the imaginable diverſities of curved and rectilinear figures, employs, in theſe acts, a mental proceſs perfectly analogous to that of the poet who feigns, *ad libitum*, characters of deities, men, and dæmons, by new combinations of thoſe moral, intellectual, and bodily qualities which he has had occaſion to behold in real life. This mathematician does, in fact, a thing preciſely ſimilar to that which is done by the common maſon or carpenter, who frames, of ſtone or wood, many new figures of building, furniture, and utenſils, not exactly ſimilar perhaps to any that he may have ſeen in ſimple unviolated nature. The chemiſt acts in the ſame manner, in every trial of the unknown combinations of any chemical ſubſtance of which he deſires to inveſtigate the character. And it is exactly thus, too, that the zoologiſt, to elucidate the natural hiſtory of any animal, places it in every diverſity of circumſtances in which he can imagine that a new quality may be diſplayed by it.

Compare the ſimpler operations in arithmetic with thoſe of logic. Multiplication is only an abbreviation of addition: addition, in every act of it, ſimply compares ſeveral different individuals, and thus aſcertains that they belong to the ſame ſpecies: every act, whether of addition or of multiplication, is preciſely that elliptical ſyllogiſm which is named an enthymeme. In every act, whether of the one or of the other, there is, in truth, an analyſis of particulars, and a general induction from them. I ſay, for inſtance, in addition, 2 and 2 are 4. This, as a ſyllogiſm, affirms, that all numbers containing equal units are equal: 4 expreſſes a certain combination of unities: 2 and 2 make together exactly the ſame combination of unities; 2 and 2 are therefore equal to 4. Let any one attend vigilantly to what paſſes in his mind in this act of addition; and he will find it to be clearly what is here ſtated. Again, this act of addition ſhows, juſt like the obſervation of a roſe-tree only in leaf, and of a roſe-tree in full flower in natural hiſtory, that 2 and 2 are, as well as 4, but combinations of the ſame ſpecies, genus, order, or claſs of unity. It is alſo an example of analyſis and induction. You analyſe when you conſider, as one unavoidably does, the conſtituent units ſeparately; you draw your general inference when you perceive the equalities. Any act in multiplication might, as to the reaſoning of it, be in the ſame manner illuſtrated. Subtraction reaſons exactly in the ſame manner as addition; but, inſtead of equalities, diſcovers diſſimilitude, and of that diſſimilitude gives a numerical definition. Diviſion combines the reaſonings of addition or multiplication with thoſe of ſubtraction. The rule of three is, moſt obviouſly, a chain of abridged ſyllogiſms; a proceſs of regular analyſis and induction; an aſcertaining of the proper ſpecies, &c. to which certain individuals belong. Purſue the reaſonings which reſpect numbers through every other variety of the accuſtomed operations; and you ſhall find, ſtill, nothing but the ſame acts of ratiocination conſtantly repeated. A ſyllogiſtic compariſon of ideas; to diſcover equality or difference; a proceſs of analyſis and induction; a reference of individuals to their reſpective ſpecies, of ſpecies to their genera, &c. &c.; are ſtill the only acts of intellect which are performed: and ſtill the mind is conſcious of no ideas but what have their origin in the obſervation of material exiſtences, and in abſtraction from theſe.

Thus it is in that part of mathematics which abſtracts quantity from extenſion, and concerns itſelf only about the relations and properties of number conſidered excluſively of figure.—Examine, on the other hand, any of the propoſitions and demonſtrations of Euclid, or any other mathematician who inveſtigates the qualities of figures in the antient form of mathematical reaſoning. Does he demonſtrate a theorem? This is only to eſtabliſh, by analyſis and induction, a general fact before unknown. Is he to ſolve a problem? This is to explain the ſecret cauſes of a general truth before known, but unaccounted for; to invent a rule, and prove it to be founded on a due knowledge of the relations of things; or to adopt a rule from others, and to ſhow by what means it is that compliance with this rule perfectly accompliſhes the end propoſed. A theorem and a problem differ from one another merely as two different modes for ſuggeſting the ſame queſtion or queſtions of the ſame ſpecies. In both, the mind analyſes and abſtracts juſt as in the proſecution of any diſcovery in natural hiſtory. The train of demonſtration in the works of all the antient mathematicians who treat of figure, is a ſeries of enthymemes or abbreviated ſyllogiſms. When I go about to demonſtrate, that the three angles of any triangle are equal to two right angles; what elſe is this, but to ſhow that the three angles of a triangle are one ſpecies of the ſubdiviſions into which the magnitude of two right angles may be marked out? Can a ſingle caſe of mathematical inquiry be pointed out, in which the reaſoning ſhall not appear to have the moſt perfect analogy with that which is uſed in every other branch of ſcience, and, indeed, in the whole buſineſs of common life? Impoſſible.

But the mathematician repreſents all magnitudes by combinations of lines incapable of exhibiting ſolid contents, or even, with accuracy, the diverſities of ſurface; and in his demonſtrations employs the letters of the alphabet to denominate thoſe lines. He does. But the abſtractions of the fancy of the poet are thus repreſented, and but in part, by the imagery of the ſculptor and the painter. The naturaliſt thus employs delineations of thoſe objects which he cannot keep conſtantly under his actual inſpection. The letters of the alphabet, and other ſigns not in any other way ſignificant, are employed in our reaſonings in every other branch of knowledge, juſt as well as in mathematics. If a few technical terms be peculiar to this branch of the ſcience of the magnitudes of figure; are not certain ſets of technical terms, in almoſt every other inſtance. much more numerous than thoſe of mathematics, peculiar reſpectively to every different ſcience, and to every different branch of art?

It may ſtill, however, be alleged, that there is a peculiarity in algebra, that branch of mathematical reaſoning in which the moderns chiefly delight. But, no: the reaſoning is, even in algebra, ſtill the ſame. The ſigns only, and the abbreviations, are ſomewhat different. Algebra refines even upon the abſtractions of arithmetic, and enables the mind to proceed through a long train of reaſoning reſpecting numerical quantities, magnitudes, or diſtinct values of any ſort, without the intervention of common language, or even of the common ſigns of number. But the object is ſtill to arrange individuals under their ſpecies, ſpecies under their genera, genera under abſtractions ſtill more comprehenſive, &c. The reaſoning is ſtill by an analytical collection of particular facts, and a concluſion to a general truth from their agreement. The ſeries of abbreviated ſyllogiſms may be juſt as clearly traced in demonſtrations by algebraic ſymbols, as in thoſe in which more tedious forms of words are employed. Algebra is but the brachygraphy of mathematics; and ſhort-hand writing implies no peculiarity of reaſoning, but merely a voluntary and conventional connection of certain ſigns with certain primary objects, whether ſounds, words written in alphabet, or diſtinct exiſtences in nature. In the higher parts of algebra, and indeed throughout all mathematics, are many abbreviations nearly ſimilar to that in which arithmetic ſubſtitutes multiplication for addition. But has not every branch of knowledge, in like manner, its abbreviations, by means of which its reaſonings are brought continually nearer to the quickneſs and certainty of intuition?

II. All that ſcience does is, not to create new exiſtences, to bring into the knowledge of man ſomething that was never before known to mind, but merely to obſerve individuals, and to form, by abſtraction, general notions by which human thought may become capable of a much greater number of individual conceptions than it could by other means poſſibly comprehend. In ſhort, all human ſcience is thus reduced to arrangement convenient to memory, and ſerving merely, as it were, to enlarge the range of the mind's eye. Now, mathematics is not a ſolitary exception: all its diſcoveries are merely diſcoveries of exiſtence and arrangement. A mathematical point is the *genus generaliſſimum* of the ſcience. Points repeated with relation merely to numerical quantity; and, on the other hand, points repeated ſo as to form extenſion; are the two great genera included immediately under that genus generaliſſimum. If we purſue lineally extended quantity into ſubdiviſions continually lower, we ſhall find next the three ſpecies of lineal extenſion by ſtraight lines, by curves, and by angles. Under the ſpecies of ſtraight lines are all the ſubordinate ſpecies of merely longitudinal extenſion, with their mutual relations and differences. The ſpecies of curves has under it all the inferior ſpecies of magnitudes or figures, which can be formed and bounded by curved lines alone, without the intervention of angles or ſtraight lines. The ſpecies of angles comprehends, as ſubordinate, all the different ſorts of ſingle angles which can be formed by the meeting of two ſtraight lines. Subordinate to the angle and the ſtraight line, jointly, are all thoſe figures complete on all ſides, which are formed by the combination of ſtraight lines and angles only. There is a fifth claſs of ſubordinate ſpecies referable to the angle, the ſtraight line, and the curve, jointly, which includes all the ſubordinate ſpecies of figures, all thoſe modes of extenſion, in which angles, ſtraight lines, and curves are combined. It were eaſy to purſue theſe ſubdiviſions continually lower, even to every conceivable diverſity, in the modes of extenſion. It might be ſhown that number is merely arrangement into genera and ſpecies, &c. with the ſame eaſe with which this is evinced of extended magnitudes. But I will not multiply examples of what is ſo obvious.

When time is made the ſubject of mathematical calculation; it is then conſidered either as numerical quantity, or as lineally extended magnitude, or as a combination of the one with the other.

Diſtances are obviouſly ſubjects of mathematical calculation, ſolely becauſe they are modes of lineal extenſion.

Nothing elſe can be brought under the mere forms of mathematics without a fiction, that it is either numerical or lineally extended quantity.

All the truths of mathematics, antient and modern, pure and mixed, might be more happily than in any other mode of expoſition unfolded in a ſyſtematic arrangement, like that under which Linnæus has diſpoſed the hiſtory of the kingdoms of nature, or the table in which the French chemiſts contrived beautifully to ſtate the truths and analogies in the Lavoiſierian ſyſtem of that ſcience.

I do not at all communicate this, Sir, as any mighty diſcovery. I ſhould only wiſh to convince ſome mathematicians that naturaliſts reaſon juſt as much as they, and preciſely in the ſame way; that mathematics, like all other branches of human ſcience, deals merely in the diſcernment of exiſtence and in arrangement; that it is not, therefore, more remote than any other ſcience from the common ſenſe and common utilities of life; that it has no right to be eſteemed the only ſcience by which the reaſoning powers are to be ſucceſsfully invigorated; that its forms may be yet advantageouſly ſimplified; and, that all its general and ſpecific truths might be very intelligibly exhibited in one ſyſtem, and in one or two grand tables.

At my firſt leiſure, Sir, I ſhall take the liberty of addreſſing you on a different ſubject; and ſhall communicate certain facts, from which the *electrical fluid* appears to me to be certainly an *oxide of caloric.*

I am, Sir, yours,
R. HERON.

London,
December 7th, 1801.

L. *Report*

L. *Report given in to the Claſs of the Mathematical and Phyſical Sciences of the French National Inſtitute on the Experiments of Volta, made the 2d of December* 1800 *.

THE firſt galvanic phænomena conſiſted of muſcular contractions excited by the contact of a metallic arc. Galvani and ſeveral other philoſophers conſidered them as produced by a peculiar electricity inherent in animal parts. Volta firſt announced that the animal arc introduced into theſe experiments ſerved only to receive and manifeſt the influence, but very little, or not at all, to produce it. The muſcular irritation, which was at firſt ſuppoſed to be the important part of the phænomenon, was, according to him, only an effect of the electric action produced by the mutual contact of the metals of which the exciting arc was formed. This opinion, which found partiſans as well as opponents, cauſed the experiments proper for ſupporting or combating it to be multiplied; and, as is always the caſe in the infancy of diſcoveries, there appeared, along with facts, a number of ſingular anomalies, which rendered it more difficult to trace out their connection, and which were then even inexplicable, becauſe they aroſe from very delicate circumſtances, the influence of which was not well known.

Such was the ſtate of this branch of philoſophy when the commiſſion delivered in to you its firſt report: its object had been to determine with accuracy the conditions proper for calling forth and modifying the galvanic effects; it did not attempt to explain them, and confined itſelf to preſenting them in that order which ſeemed moſt proper. At that period we were not acquainted with the reſearches by which C. Volta, purſuing the path he has opened, endeavoured to connect with his firſt diſcovery all the phænomena exhibited by galvaniſm. He has ſince made known a great many others, equally important, which he has connected together by a very ingenious theory; and if any thing ſtill remains to be done to determine with exactneſs the laws of this ſingular action, and to ſubject it to rigorous calculation, the principal facts at leaſt, which ought to ſerve as the baſis, ſeem to be invariably fixed.

Your commiſſion propoſes at preſent to give an account of theſe fundamental experiments, and the method in which

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* The commiſſion charged with this report conſiſted of Laplace, Coulomb, Hallé, Monge, Fourcroy, Vauquelin, Pelletan, Charles, Briſſon, Sabathier, Guyton, and Biot.

Volta employed them for the eſtabliſhment of his theory. It is much indebted to that learned man for the readineſs with which he repeated them, ſeveral times, before the commiſſioners; who can thus vouch for their truth and correctneſs. The principal fact, that from which all the reſt may be deduced, is as follows:

If two different metals, inſulated, and having only their natural quantity of electricity, are brought into contact, when removed from that contact they are found in different ſtates of electricity: one is poſitive, and the other negative.

This difference, very ſmall at each contact, being accumulated in an electric condenſer, becomes ſufficiently ſtrong to cauſe the electrometer ſenſibly to diverge. The action is not exerciſed at a diſtance, but only on the contact of different metals; it ſubſiſts as long as the contact continues, but its intenſity is not always the ſame. It will here be ſufficient to take as examples copper and zinc. In their mutual contact the copper becomes negative, and the zinc becomes poſitive.

Having proved the development of metallic electricity independently of any moiſt conductor, C. Volta introduces theſe conductors.

If a metallic plate be formed of two pieces, one of zinc and the other of copper, ſoldered end to end; if the extremity of the zinc be held between the fingers; and if the other, which is of copper, be made to touch the upper plate of the condenſer, which is alſo of copper, the latter will be charged negatively. This is evident from the preceding experiment.

On the other hand, if the copper extremity be held between the fingers, and if the other extremity, which is of zinc, be made to touch the upper plate of the condenſer, which is of copper, when the contact is deſtroyed, and the upper plate is removed, it has not acquired electricity, though the lower plate communicates with the common reſervoir.

But if paper moiſtened with pure water, or any other moiſt conductor, be placed between the upper plate and the zinc extremity, the condenſer becomes charged with poſitive electricity. It becomes charged alſo, but negatively, when the upper plate, covered with a moiſt conductor, is touched by the copper extremity, holding the zinc extremity between the fingers. Theſe facts are inconteſtable; they have been verified by the commiſſion.

The manner in which C. Volta explains them, and refers them to the preceding, is as follows:

Metals, ſays he, and perhaps all the bodies in nature, exerciſe, as has been ſaid, a reciprocal action on their reſpective electricities at the moment of contact. When the metallic plate is held by its copper extremity, a part of its electric fluid paſſes into the oppoſite plate, which is of zinc; but if this zinc is in immediate contact with the condenſer, which is alſo of copper, the latter tends to diſcharge itſelf of its fluid with equal force, and the zinc cannot tranſmit any to it: after the contact, therefore, it muſt be found in its natural ſtate. On the other hand, if a piece of moiſt paper be placed between the zinc of the plate and the copper plate of the condenſer, the moving power of the electricity, which exiſts only at the time of contact, is deſtroyed between theſe two metals: the water, which ſeems to enjoy this property in a very weak degree in regard to metallic ſubſtances, checks only a very little the tranſmiſſion of the fluid from the zinc to the condenſer, and the latter may be charged poſitively.

In the laſt place, when the condenſer is touched with the extremity of the plate which is of copper, the moiſt paper interpoſed, the proper action of which is exceedingly weak, does not prevent the metallic plate from conveying a part of its metallic electricity into the plate of zinc: then, by deſtroying the contact, the condenſer is found charged negatively.

From this theory it is eaſy to explain the Voltaic pile. To do it with more ſimplicity, we ſhall ſuppoſe that it is formed on an inſulator, and we ſhall repreſent by unity the exceſs of electricity which a piece of zinc ought to have over a piece of copper immediately touched by it *.

If the pile is compoſed only of two pieces, a lower one of copper and the ſuperior of zinc, the electric ſtate of the former will be repreſented by $-\frac{1}{2}$, and that of the ſecond by $+\frac{1}{2}$.

If a third piece be added, which muſt be of copper, to produce a diſplacement of the fluid, the lower piece of zinc muſt be ſeparated by a piece of moiſt paſteboard; it muſt then acquire the ſame electric ſtate as the latter; at leaſt, it will be ſo if we neglect the action proper to the water, which appears very ſmall, and perhaps alſo the very weak reſiſtance

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* The quantities of electricity accumulated in a body beyond its natural ſtate are, *cæteris paribus*, proportional to the repulſive force with which the molecule of the fluid tend to ſeparate from each other, or to repel a new molecula which one may attempt to add to them. This repulſive force, which in free bodies is balanced by the reſiſtance of the air, conſtitutes what we call the *tenſion* of the fluid: a tenſion which is not proportioned to the divergency of the ſtraws in Volta's electrometer, nor to that of the balls in Sauſſure's, and which cannot be accurately meaſured by means of the electric balance.

which that liquid, as an imperfect conductor of electricity, can oppoſe to the communication. The apparatus being inſulated, the exceſs of the upper part can be acquired only at the expenſe of the piece of copper which is below: the reſpective ſtates of theſe pieces will be no longer the ſame as in the preceding experiment, and will become,

In the lower piece, which is of copper, $-\frac{2}{3}$; in the ſecond, which touches it, and which is of zinc, $-\frac{2}{3}+1$, or $\frac{1}{3}$.

The third, which is of copper, and which is ſeparated from the preceding by a piece of moiſt card, will have the ſame quantity of electricity, that is to ſay, $+\frac{1}{3}$; and the ſum of the quantities of electricity loſt by the firſt piece and acquired by the other two, will be ſtill equal to zero, as in the caſe of the two pieces.

If we add a fourth piece, which will be of zinc, it muſt have an unit more than that of the copper over which it is immediately placed: as this exceſs can be acquired only at the expenſe of the inferior pieces, ſince the pile is inſulated, we ſhall have:

For the lower piece, which is of copper, -1: for the ſecond piece, which touches it, and which is of zinc, 0; that is to ſay, it will be in its natural ſtate;

For the third piece, which is of copper, and which is ſeparated from the former by a piece of moiſt card, 0; it will be alſo in its natural ſtate;

Laſtly, for the upper piece, which is of zinc, and which is in contact with the preceding, -1.

Purſuing the ſame reaſoning, the electric ſtate of each piece of the pile will be found by ſuppoſing it inſulated, and formed of any number of elements; the quantities of electricity will increaſe for each of them, from the baſe to the ſummit of the column, according to an arithmetical progreſſion, the ſum of which will be equal to zero.

For the greater ſimplicity, if we ſuppoſe that the number of the elements is even, it will be eaſy to aſcertain, by a very ſimple calculation,

That the lower piece which is of copper, and the upper piece which is of zinc, muſt be equally electrified, one more and the other leſs; and the caſe will be the ſame in regard to the pieces taken at an equal diſtance from the extremities of the pile.

The electricity, before it paſſes from poſitive to negative, will become null; and there will always be two pieces, one of zinc and the other of copper, which will be in the natural ſtate. They will be in the middle of the pile: this is what has been ſeen in the caſe of four pieces, for example.

Let

Let us now ſuppoſe that the communication is eſtabliſhed between the lower part of the pile and the common reſervoir: it is evident that in this caſe the lower piece of copper, which is negatively electrified, will tend to reſume from the ground what it has loſt; but its electric ſtate cannot be changed without that of the upper pieces varying, ſince the electric difference of the one from the other muſt always be the ſame in the ſtate of equilibrium. It is neceſſary, then, that all the negative quantities of the lower half of the pile ſhould be neutralized at the expenſe of the common reſervoir; and then it will reſult:

That the lower piece, which is of copper, will have the ſame degree of electricity as the ground, which ſhall be repreſented by 0;

The ſecond piece, which is of zinc, and which immediately touches the preceding, will have + 1;

The third, which is of copper, and which is ſeparated from the lower one of zinc by a piece of moiſt paſteboard, will have, like it, + 1;

The fourth, which is of zinc, and which touches the preceding, will have + 2;

And the quantities of the electricity of the different elements will increaſe in this manner according to an arithmetical progreſſion.

If the ſummit of the pile be then touched with one hand, and its baſe with the other, theſe exceſſes of electricity will be diſcharged through the organs into the common reſervoir; and will give a ſhock the more ſenſible, as, this loſs being repaired at the expenſe of the ground, there muſt thence reſult an electric current, the rapidity of which, greater in the interior of the pile than in the organs, which are imperfect conductors, permits the interior part of the pile to reſume a degree of tenſion approaching that which it had in the ſtate of equilibrium.

The communication being ſtill eſtabliſhed with the common reſervoir, if the ſummit of the pile be brought into contact with the upper plate of a condenſer, the lower one of which touches the ground, the electricity, which at that extremity was in a very weak degree of tenſion, will paſs into the condenſer, where the tenſion may be conſidered as null; but the pile being inſulated only at the expenſe of the common reſervoir, the new quantities of electricity, recovered by the upper plate, will paſs into the condenſer like the preceding, and will be there, at length, accumulated in ſuch a manner, that, by ſeparating the collecting plate, very ſenſible electrometric ſigns, and even ſparks, may be extracted.

In regard to the limits of this accumulation, it is obvious that it depends on the thickneſs of the ſmall ſtratum of gum which ſeparates the two plates of the condenſer; for, as in conſequence of this thickneſs the electricity accumulated in the collecting plate cannot act but at a diſtance on that of the lower plate, it is always more conſiderable than that which forms an equilibrium with it in the latter; and hence there reſults in the collecting plate a ſmall tenſion, which has here for its limit the tenſion exiſting in the upper part of the pile.

As the electricity of the column is accumulated in the condenſer, it will be accumulated in like manner in the interior of a Leyden flaſk, the exterior of which communicates with the common reſervoir; and ſince, in proportion as the pile diſcharges itſelf, it is recharged at the expenſe of the ſame reſervoir, the flaſk will be equally charged whatever be its capacity; but its interior tenſion can never exceed that which takes place at the ſummit of the pile: if the bottle be then removed it will give a ſhock correſponding to that degree of tenſion; and this is what is confirmed by experience.

Such muſt be the ſtate of things, neglecting, as very ſmall, the action proper to the water on the metals; ſuppoſing,

1ſt, That the tranſmiſſion of the fluid takes place from one pair to the other in the inſulated pile through the pieces of moiſt paper by which they are ſeparated, even when there exiſts no communication between the two extremities of the column:

2d, That the exceſs of electricity which the zinc takes from the copper is conſtant for theſe two metals, whether they are in their natural ſtate or not.

C. Volta ſupports the firſt propoſition by an experiment which we have already mentioned, and in which the condenſer is charged on bringing the collecting plate, covered with a piece of moiſt paper, into contact with the copper extremity of a metallic plate, the other or zinc extremity of which is held between the fingers.

In regard to the ſecond ſuppoſition, it is more ſimple than might be imagined; but a ſeries of very nice experiments, which we had not opportunity of making, would be neceſſary to aſcertain how far it is agreeable to nature.

Hitherto, for the ſake of preciſion, we have ſuppoſed the pile to be compoſed of copper and zinc: the ſame theory, however, is applicable to any two metals whatever; and the effects of the different kinds of apparatus which they will ſerve to form, will depend on the differences of the electricity which would be eſtabliſhed between them at the moment of contact.

What we have ſaid extends alſo to all other bodies between which there may exiſt an analogous action: thus, though this action may appear in general very weak between liquid and metallic ſubſtances, there are however ſome, ſuch as the alkaline ſulphurets, the action of which with the metals becomes very ſenſible: the Engliſh chemiſts, therefore, have been able to ſupply by theſe ſulphurets one of the metallic elements of the pile; and before them profeſſor Pfaff, of Kiel, employed them for this purpoſe in his experiments.

In this reſpect, C. Volta has diſcovered between the metallic ſubſtances a very remarkable relation, which renders it impoſſible to conſtruct a pile with theſe ſubſtances alone. We ſhall here explain them according to his account, but we have had no opportunity of confirming them.

If the metals be arranged in the following order—ſilver, copper, iron, tin, lead, zinc,—each of them will become poſitive by the contact of that which precedes it, and negative with that which follows it: the electricity will paſs then from the ſilver to the copper, from the copper to the iron, from the iron to the tin; and ſo on.

Now the property in queſtion conſiſts in this, that the moving force of the ſilver to the zinc is equal to the moving forces of the metals comprehended between them in the ſeries: hence it follows that, by placing them in contact in this order, or in any other at pleaſure, the extreme metals will be always in the ſame ſtate as if they immediately touched each other; and conſequently, ſuppoſing any number of elements whatever thus diſpoſed, and of which the extremities are for example ſilver and zinc, we ſhall have the ſame reſult as if theſe elements were formed of theſe two metals; that is to ſay, there will be no effect, or it will be that which would have been produced by one element.

It has hitherto appeared that the preceding property extends to all ſolid bodies, but it does not ſubſiſt between them and the liquids: hence it happens that a pile may be conſtructed by the medium of the latter. From this reſults the diviſion which Volta makes of conductors into two claſſes; the firſt comprehending ſolid bodies, and the ſecond liquids.

It has not yet been poſſible to conſtruct a pile but by a proper mixture of theſe two claſſes: it cannot be done with the firſt alone, and we are not yet ſufficiently acquainted with the mutual action of the bodies which compoſe the ſecond, to determine whether the caſe is the ſame in regard to them.

We have ſuppoſed that the moiſt paſteboards placed between the elements of the pile are moiſtened with pure water. If inſtead of water a ſaline ſolution be employed, the ſhock will become incomparably ſtronger; but the tenſion indicated by the electrometer does not appear to increaſe in the ſame ratio. C. Volta has proved to us this fact by the help of his apparatus formed of cups, by pouring ſucceſſively into them pure water and acidulous water.

He concludes from this experiment that acids and ſaline ſolutions favour the action of the pile, chiefly becauſe they increaſe the conducting property of the water with which the paſteboard is moiſtened. In regard to the oxidation, he conſiders it as an effect which eſtabliſhes a more intimate connection between the elements of the pile, and thus contributes to render its action more energetic.

Such nearly is the ſubſtance of the theory of C. Volta reſpecting that electricity called *galvanic*. His object has been to reduce all the phænomena of it to one, the exiſtence of which is now fully confirmed; it is the development of metallic electricity by the mutual contact of metals. It ſeems to be proved by theſe experiments, that the peculiar fluid to which muſcular contractions and the phænomena of the pile were for ſome time aſcribed, is nothing elſe than the common electric fluid put in motion by a cauſe reſpecting the nature of which we are ignorant, though we ſee its effects.

Such is the fate of the ſciences, that the moſt brilliant diſcoveries only open a more extenſive field for new reſearches. After having aſcertained and eſtimated, as we may ſay, by approximation the mutual action of the metallic elements, it remains to determine it in a rigorous manner, to diſcover whether it is conſtant for the ſame metals, or whether it varies with the quantities of electricity they contain, and with their temperature. We muſt aſcertain with the ſame preciſion the peculiar action which the liquids exerciſe on each other and on the metals. It will be then that we can eſtabliſh our calculation on exact data; that we can diſcover the real law followed by the diſtribution and motion of the electricity in Volta's apparatus, and complete the explanation of all the phænomena it exhibits. But theſe nice reſearches require inſtruments more correct than any yet invented by philoſophers to meaſure the force of the electric matter.

In a word, it remains to examine the chemical effects of the current of electricity, its action on the animal economy, and its relation with the electricity of minerals and fiſhes; reſearches which, from the facts already known, cannot fail to be of very great importance.

When a ſcience, already far advanced, has made an important ſtep, new connections are formed between the branches of which it is compoſed: we are always fond of looking backwards to meaſure the field it has paſſed over, and to ſee how the human mind has advanced in it. If we thus go back to the birth of electricity, we ſhall find it, at the commencement of the laſt century, confined merely to the phænomena of attraction and repulſion alone. Dufay firſt aſcertained the conſtant laws to which they are ſubjected, and explained their apparent ſingularities. His diſcovery of two kinds of electricity, reſinous and vitreous, laid a foundation for the baſis of the ſcience; and Franklin, by preſenting it under a new point of view, made it the foundation of his theory, to which all the phænomena, even that of the Leyden flaſk, were naturally referred. Æpinus completed the proofs of this theory, brought it to perfection by applying to it calculation, and by the help of analyſis attained to thoſe phænomena which C. Volta has ſo happily employed in the condenſer and electrophorus. The rigorous law of electric attraction and repulſion, ſtill wanting, was eſtabliſhed by exact experiments, and, connecting itſelf with that of magnetiſm, was found to be the ſame in regard to celeſtial attractions. It is well known that C. Coulomb is the author of this diſcovery.

At length appeared the galvanic phænomena, ſo ſingular in their progreſs and ſo different in appearance from every thing known before. At firſt, a peculiar fluid was created to explain them; but C. Volta, by a ſeries of ingenious experiments conducted with ſagacity, propoſes to refer to one cauſe the development of metallic electricity; to employ them for the conſtruction of an apparatus which will allow their force to be augmented at pleaſure; and connects them, by his reſults, with important phænomena of chemiſtry and the animal economy.

In conſequence of a requeſt made by one of your members, and which you referred to a commiſſion, we propoſe that the gold medal of the Inſtitute be awarded to C. Volta, as a teſtimony of the ſatisfaction of the claſs for the noble diſcoveries with which he has enriched the theory of electricity, and as a mark of its gratitude for having communicated them to it.

LI. *Thoughts on the ſuppoſed Variations in the Axis and Poles of the Earth. By Profeſſor* BODE, *of Berlin* *.

IT is highly worthy of remark, that, notwithſtanding the harmony which prevails in the courſe of the planets round the ſun, and in their rotation, as well as in regard to the parallelīſm of their axes, great variations are found in the inclination and poſition of the latter, and in the periods of the rotation of theſe bodies.

This depends on the different influence which the ſun has on a planet during its rotation, or the variation of its aſtronomical ſeaſons, and on the inclination of its axis in regard to its orbit; for the ſun, in his courſe from the one tropic to the other, paſſes over an arch equal to twice the complement of this inclination; and therefore the leſs the inclination, he muſt advance nearer to the poles, and employ much leſs time in proceeding from the equator towards either tropic.

Now, as it is known from obſervations that there are ſome planets the axes of which have a much greater or much leſs inclination than that of our earth, the great variations which muſt naturally thence reſult in regard to the beneficent influence of the ſun's light and heat, will give a quite different nature to their fields, productions, and inhabitants.

The inclination of the axis of our earth, which is $66\frac{1}{2}$ degrees, ſeems to be well adapted for promoting its fertility and rendering it habitable, as the ſun employs ſix months in moving from the equator towards either pole, and again returning; by which means the effect of his rays in producing that heat and light ſo indiſpenſably neceſſary to the animal, vegetable, and mineral kingdom, increaſes and decreaſes in a much ſlower and gradual manner. The two temperate zones occupy the greater part of the earth's ſurface, and theſe remain habitable as far as poſſible towards either pole.

The direction and inclination of the axes of the different planets are as little proportioned to their different diſtances from the ſun, as their periods of rotation, ſize, denſity, maſs, and ſphericity; as is well known by obſervations which have been made. Now, as there muſt be ſufficient grounds for this arrangement, it may be aſcribed, in my opinion, to the matters of different ſpecific gravity of which the earth and the other planets are compoſed, and to their mixture; in conſequence of which, immediately after their formation, their hemiſpheres on each ſide of their orbit, which always divides the body in the middle, were put in perfect equilibrium.

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* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. ii. 1799.

Our earth as well as every other planet is carried round the ſun by the radius vector, (the ſtraight line drawn from the planet to the ſun,) as the centripetal forces continually act according to that line, or in a perpendicular direction. But this line of attraction, during the revolution of the earth, on account of the continual paralleliſm of its axis, forms with the latter very different angles; and yet the duration and period of the rotation remain unaltered. This great difference in the angles has therefore no influence whatever in this reſpect; and it appears that there exiſts ſome other great power capable of preſerving the direction and inclination of the axis unchanged, and which, in my opinion, may be aſcribed alſo to the ſun.

Our earth during its annual courſe, revolving from weſt to eaſt, turns in ſucceſſion every part of its ſurface to the ſun, and at the ſame time revolves from eaſt to weſt on a line paſſing through its centre perpendicular to the plane of its orbit; or paſſing through the poles of the ecliptic, as a ſecond axis, for the purpoſe of conſtantly maintaining the paralleliſm of its proper axis of rotation. Now, if it can be admitted that the ſun produces this revolution of the earth in the plane of the ecliptic correſponding with the arch of its orbit, this, in my opinion, is a more natural explanation of the paralleliſm of the earth's axis than when it is conſidered merely as the conſequence of a certain eſtabliſhed poſition of it incapable of alteration; as this principle is applicable only to the action of mechanical powers on bodies that move and revolve in a ſtraight line; whereas the ſun is continually drawing our earth from the ſtraight line, and carrying it around it in a cruciform manner.

According to this theory, we may eaſily conceive how it is poſſible that a planet the axis of which, by the equilibrium of its heterogeneous parts eſtabliſhed at its creation, is put into a certain direction, ſubjected to certain laws, may, by the powerful influence of the ſun, be made to deviate from it ſo that its axis ſhall aſſume another poſition and inclination, or, in other words, change its poles.

Our earth, in conſequence of ſlow or ſudden variations of the laſt kind, would produce in the heavens the following phænomena:

1ſt, If the inclination of the earth's axis ſhould change, the zodiac would be ſtill the ſame, but the obliquity of the ecliptic would be greater or leſs; the equator would paſs through other ſtars; and, in this caſe, all countries lying under the ſame parallels of latitude would experience a like change in regard to the duration of the ſeaſons.

2d, If the axis of the earth, without changing its inclination, ſhould acquire a new direction towards ſome other quarter of the heavens, the two points ♈ and ♎, where the ecliptic interſects the equator, and all the other circles connected with it, would paſs through other points of the ecliptic. This would make no change in the duration of the ſeaſons, or the ſtate of the ſun at noon.

3d, If the inclination as well as poſition of the axis ſhould be changed, a reſult compounded of both the above caſes would take place.

4th, But if the earth ſhould be carried around the ſun in another plane, the reſult would be a new apparent courſe of the ſun. The ſeaſons would remain the ſame, or acquire ſome change in their duration, according as the axis ſhould be inclined as at preſent, or in a different manner towards the new orbit, and all the countries of the earth would participate in this change. Theſe four caſes, however, have no influence on the elevation of the pole of any place.

5th, On the other hand, ſhould the poles of the earth change their place, the obliquity of the ecliptic would alſo be changed, the equator would paſs through other countries, and the elevation of the pole of theſe countries would be changed. This change of the pole, however, can take place only in the direction of ſome meridian; and conſequently from north to ſouth, and from ſouth to north. By theſe means one half of the equator would be carried over northern and the other over ſouthern countries; and therefore all countries would not equally participate in the changes thence occaſioned in the duration of the ſeaſons; at leaſt, all countries lying under the ſame ſouthern or northern zones could not come together into the torrid zone, or into an uniform ſituation.

If the alteration of the pole ſhould take place in the direction of the meridian that paſſes through the ſolſtices, neither the ecliptic nor the different quarters of the earth would experience any change; the obliquity of the former only would be altered as well as the poſition of the equator in proportion to the above alteration. Should the alteration of the pole take place in the direction of any other meridian, a change would be produced in the different quarters of the globe, the obliquity of the terreſtrial equator, and the ſigns of the ecliptic. In the laſt place, ſhould the pole move along the meridians paſſing through either of the equinoctial points, the above phænomena would take place, and the points of the ecliptic, 0° ♈ and ♎, would be transferred to ♋ and ♑, and north and ſouth to eaſt and weſt.

A queſtion now ariſes, whether any traces exiſt on the ſurface of our globe which ſeem to ſhow that the inclination of the earth's axis has ever changed either ſuddenly or progreſſively, or whether the pole has altered its place? On the firſt view, it would appear that this queſtion ought to be anſwered in the affirmative.

Many naturaliſts and geologiſts, in order that they might more eaſily explain the general deluge, and why marine productions are often found at a conſiderable depth under the earth's ſurface, or on the tops of high mountains; how the remains of auſtral plants, and of terreſtrial and aquatic animals, could have been conveyed to the northern regions, and buried there in the boſom of the earth; how the different ſtrata of mountains have been formed, and other phænomena of the like kind, have been able, as is well known, to contrive no better hypotheſis than to ſuppoſe that ſome derangement of the poles of the earth, or changes in the inclination of its axis, muſt have taken place in former times.

But, in my opinion, the determination of this queſtion belongs rather to aſtronomers than to geologiſts, becauſe by the former it may be conſidered under a more general point of view, viz. what connection exiſts between the poſition of the earth's axis and the ſun, and the attractive power in the ſolar ſyſtem; and what obſervations have been made in regard to the poſition and direction of their axes, from which, by analogical reaſoning, we may explain the relation between cauſes and effects when ſuch changes take place.

The ableſt aſtronomers and geometricians have ſhown that the ſmall annual receſſion of the equinoctial points to the weſt, of about 51 ſeconds, and the revolution of the earth's axis around the poles of the ecliptic, which thence follows in the courſe of 25,700 years, ariſe from the united action of the attractive power of the ſun and moon on the ſpheroidal form of the earth. As long, therefore, as this attraction of the ſun and moon acts in an uniform manner, it ſeems to be impoſſible that any continued or very perceptible changes in regard to the earth's axis can take place.

It is, however, found by obſervations, that in the courſe of many centuries an alteration, though very ſmall, has taken place in the inclination of the earth's axis. At preſent it is 23 minutes greater than it was in the time of Hipparchus, that is, above 2000 years ago.

In conſequence of this obſervation it has been apprehended that the ecliptic may, at ſome future period, coincide with the equator; the ſun, of courſe, be continually in the equator; and the axis of the earth acquire a perpendicular direction; by which means the cold of the polar regions and in the temperate zones will become greater, and conſequently the fitneſs of the earth for being inhabited, as well as its culture and fertility, muſt decreaſe.

But La Grange and La Place have freed us from any uneaſineſs on this head, as they have proved, by ingenious calculations and concluſions, that this ſmall change in the inclination of the earth's axis is an effect of the mutual attraction of the planets on the orbit of our earth; that ſince the time of Hipparchus it has contributed in an uniform manner to the decreaſe of the obliquity of the ecliptic, or the increaſe of the above inclination; that it does not always continue; but that, properly, the above ſuppoſed axis of the earth's rotation perpendicular to the ecliptic, in conſequence of this action of the planets, moves in very long periods around the poles of the orbit of the earth which lie in the neighbourhood of the poles of the ecliptic.

According to theſe obſervations, the variation in the obliquity of the ecliptic is merely a nutation of the earth's axis, which, ſince the time of Hipparchus, has occaſioned a very ſmall decreaſe in it, but will afterwards become ſtationary, and then produce an increaſe.

Schubert, of Peterſburgh, has found, by La Grange's formula, that the obliquity of the ecliptic in the period of 65,000 years always remains between 20° 43′ and 27° 45′. At preſent it is about 43 minutes leſs than the mean of the above two quantities, and will ſtill decreaſe, for 4900 years, to 22° 53′; after which it will again increaſe.

Hence may be deduced very important conſequences in regard to the durable ſtate of our earth, on which diſtant bodies, in the courſe of its annual revolution, exerciſe an action; but theſe bodies, on account of their ſituation and great diſtance, are able to produce only very ſmall and periodical nutations, by which the maintenance of the whole is promoted, and the earth preſerved from great and diſaſtrous changes. As long as the preſent conſtitution of the ſolar ſyſtem remains the ſame, theſe ſmall and harmleſs periodical nutations will take place.

But if the earth's axis and its poles are ſecured by ſuch powerful bonds from all ſudden or progreſſive changes, thoſe apparent changes and revolutions which we obſerve at and under the ſurface of our earth muſt be referred to other cauſes than a change in the earth's axis, or diſplacement of its poles; and this will be ſtill more evident when it is conſidered how trifling ſuch changes are in regard to the whole earth, and whether its centre of gravity could by theſe means ſuffer any alteration.

A curſory view of geology will ſhow how little we know of the interior parts of the earth, and how ſmall a portion of it is ſubject to our dominion and reſearch. Of the 21,951,022 ſquare leagues, which is the extent of the earth's ſurface, the lands that riſe above the ocean comprehend only about one-third, or ſeven millions. Now if we ſuppoſe, which perhaps ought to be taken into account, that the height of the land above the ocean is 6000 feet, or 1[illegible] mile, (though few lands are ſo high;) and that the depth of the ocean is the ſame; this in a globe of one inch diameter would ſcarcely amount to the fiftieth part of a line, or the fifth part of a moderate ſized grain of ſand; and this whole external cruſt, of 6000 feet in thickneſs, contains only the 1100th part of the whole number of cubic miles, which form the ſolid content of the earth.

Now all thoſe traces of revolutions in the earth which geologiſts have been able to obſerve, have been found either in the above-mentioned cruſt, into which no one has ever penetrated above a fourth part of that depth, (for the greateſt depth to which mines have been dug is not more than 1500 feet;) or in the bowels or at the top of moderate ſized mountains, the height of which on a globe a foot in diameter would not amount to a grain of ſand.

Can it be ſuppoſed that revolutions have taken place in this thin cruſt of the earth capable of altering its centre of gravity, and at the ſame time the place of its poles and its axis, ſo much as ſome have ſuppoſed, in order to account for various phænomena, ſuch as that of the bones of the elephant, &c. being dug up in Germany, and even in Siberia; or if, by ſome violent action of the powers of nature in the univerſe, mountains have been ſeveral times overturned, and ſea and land changed their ſituation; would theſe cataſtrophes, which muſt have occaſioned great devaſtation among the human race at the periods when they took place, have been able in any manner to change the poſition of the earth's axis and of its poles? By no means: they muſt have been of much leſs conſequence, in that reſpect, than the devaſtation which an inſect would occaſion in a globe a foot in diameter by gnawing the paper with which it is covered: and who will aſſert that the centre of gravity of ſuch a globe would by theſe means be deranged?

A change in the poſition of the earth's axis, or of its poles, can be ſuppoſed to take place only when the whole maſs of which it conſiſts is entirely inverted, and its homogeneous and heterogeneous parts mixed together in confuſion. But in this caſe, the exterior cruſt of the earth which we inhabit would be wholly transformed; mountains, countries, and ſeas, would change their places, and be conveyed to the height or depth of a hundred miles perhaps, and more: and how is it poſſible that man, who can penetrate only to a very ſmall depth in the earth, ſhould be able to obſerve ſuch remains of the old world?

Beſides, the globe, which is of a ſpheroidal form, revolves round its leſs diameter, which is five miles ſhorter. Now, if the angle of the inclination of this axis ſhould be ſuddenly or gradually changed, its rotary motion would not be deranged, but the obliquity of the ecliptic would be expoſed to changes, of which, according to obſervations made at all periods, no traces have been found. But if the poles of the earth ſhould leave their place, a new axis would be produced, the direction of its diurnal rotation would form another equator, and the ſpherical form would be changed. But the immenſe centrifugal force would be continually producing the moſt dreadful revolutions in the land and ſea, and the remains of the antient population would be buried ſo deep in the earth as to eſcape the reſearches of ſucceeding generations.

The poles of our earth, at preſent, ſeem to occupy the moſt commodious poſitions; for as, on account of the cold, they are uninhabitable to beings of our ſpecies, whatever be the inclination of the earth's axis, the ocean has been aſſigned them as their place, and all the lands of the earth are ſituated around them, that, during the daily rotation and annual revolution of the earth round the ſun, its ſurface may as much as poſſible be expoſed to the beneficent effects of that luminary in his courſe from the equator towards the poles. If we ſhould transfer the north pole, for example, to the middle of Aſia, the ſouth pole would fall in South America, and an immenſe tract of cultivated land, on account of the cold, would be converted into uninhabitable deſerts. It is, however, highly probable, that the preſent poles, ſince the formation of the earth, as we are at preſent acquainted with its ſurface, conſiſting of ſea and land, have always had the ſame inclination.

As far as certain aſtronomical obſervations go back, and the antiquity of theſe amounts to 4450 years, when a Chineſe aſtronomer obſerved (exactly according to the preceſſion of the equinoxes) the ſtar α in the northern Dragon in the neighbourhood of the north pole, no phænomena have taken place in the heavens which give reaſon to ſuppoſe any conſiderable variation in the inclination of the earth's axis, or in the place of the poles. What Herodotus therefore ſays, reſpecting the legends of the Egyptian prieſts, is mere fables; for if the ſun, in the courſe of 11000 years, actually changed the place of his riſing and ſetting three times, ſo often muſt the place of the poles, and conſequently the climate of Egypt, have been changed. But ſuppoſing this to have been the caſe, how could the inhabitants of ſo level a country preſerve themſelves from utter deſtruction during the repeated transformation of land into water, which muſt have been the conſequence of theſe changes? By a mere increaſe or decreaſe in the preſent inclination of the earth's axis, which is 66½°. Germany, for example, could never be transferred into what is called the torrid zone. In the firſt caſe, if the inclination ſhould be changed to 90°, the ſun in ſummer would be removed about 23½° further towards the ſouth from that country. In the ſecond caſe, if the inclination ſhould decreaſe to 30°, the ſun would go to the diſtance of 60° from the equator, and in ſummer paſs twice over Germany in a perpendicular direction; but in winter he would be inviſible for a month, and the cold there would be much more intenſe than at preſent. On this account, neither the auſtral plants nor elephants could ever thrive in ſuch a climate.

If Germany was ever ſituated in the torrid zone, or near the equator, the north pole muſt have been ſituated in the Pacific ocean between Aſia and America, and the ſouth pole at the ſouthern extremity of Africa, more than 3000 miles from the places which they occupy at preſent. The north-eaſt part of Aſia muſt in that caſe have been nearer the north pole than at preſent, and Siberia in the temperate zone.

But as the bones of the elephant have been found under the earth in the northern parts of Siberia as well as in Germany, both countries muſt have been ſituated in the torrid zone: but, in this caſe, the north pole muſt have been ſituated in California, and the ſouth pole in Madagaſcar, above 4000 miles from their preſent place. If the poles were ever in that ſituation, it may be aſked, If elephants exiſted then in Siberia and Germany, where were the foreſathers of thoſe which now exiſt in the Eaſt Indies and Africa? as theſe countries at that period muſt have been in the middle of the ſouth temperate zone, or partly in the frigid zone.

Thus there are manifeſt contradictions in ſo readily ſuppoſing a diſplacement of the pole, and when the revolutions which ſuch a change muſt have produced, in regard to the ſurface of the earth and ſea, are not taken into conſideration.

A natural conſequence follows. If our northern diſtricts of the earth were ever ſituated in the torrid zone, a part of the preſent torrid zone (for it is not poſſible that this ſhould have been the caſe with the whole of it) muſt have occupied the ſouth temperate zone, or the frigid zone. If elephants could exiſt in thoſe ſouthern diſtricts, at that time forming the temperate and frigid zone, or if Germany and Siberia were ſuddenly tranſported more than four thouſand miles towards the north pole, could theſe animals withſtand this general revolution of the ſurface of the earth and ocean? Or, during this cataſtrophe, were ſome of theſe ponderous animals tranſported over the land and ſea to their preſent place of abode, in order to propagate their breed?

Theſe queſtions cannot be anſwered; but are unneceſſary, if we are convinced, from the principles here explained, that ſuch diſplacements of the poles of the earth are not admiſſible.

But as the bones of elephants and the remains of auſtral plants are found under the earth in northern countries, this may be explained in the moſt natural way, by ſuppoſing that formerly a ſpecies of theſe large animals and auſtral plants exiſted in the temperate north latitudes; for it is certain that many petrified ſhells and plants, as well as the impreſſions of inſects, are found under the earth, the originals of which are unknown; or that the preſent temperate zone enjoyed in the antient world a warmer phyſical temperature.

Are there not ſome proofs, generally ſpeaking, that the maſs of the cold in the north increaſes, and that the ice is more and more accumulated in the neighbourhood of the north pole; that even our vegetables do not poſſeſs the ſame degree of perfection, and betray ſymptoms of degeneration, though the ſmalleſt diſplacement has never taken place in the poles of the earth, and no change in the inclination of the earth's axis that could have an influence on the climate? Is not the cold more widely diffuſed in the ſouthern hemiſphere than in the northern, though both are equally expoſed to the ſun, and though the ſun in ſummer is many thouſand miles nearer the former than the latter?

The phyſical climates of the earth however, and particularly the two temperate and two frigid zones, as they are far more expoſed to the varied influence of the ſun's rays than the torrid zone, appear to have been ſubjected to great variations at long intervals. But theſe depend much on local and temporary circumſtances, and are not determined merely by the different effects of the ſun's influence, in conſequence of his annual return. They are at the ſame time, owing to the chemical fermentation in the mixture of the component parts, occaſioned by the acceſſion of heat; to the ſolution and decompoſition of theſe ſubſtances ſo different in their nature, evaporated from the ſurface of the earth, whether land or water, which is filled with animal and vegetable bodies; and to theſe vapours being in part conveyed into the atmoſphere, and there converted into other matters, and being again ſent back to it in the form of fertilizing moiſture.

In the laſt place, all our obſervations have hitherto ſhown that without the globe of the earth there is no power capable of producing ſuch irregularities in its poles and its axis.

It was long believed indeed that comets were capable of producing ſuch revolutions. Who knows, ſaid ſome, whether one of theſe bodies may not have approached too near to our earth; and, being furniſhed with too great attractive power, may have deranged its axis or its pole, or dragged its train over us; and, ſetting every thing on fire, may have laid waſte ſea and land, altered the earth's centre of gravity, and occaſioned a general devaſtation?

But our preſent more accurate knowledge of the orbits and nature of theſe celeſtial bodies will never admit of any ſuppoſition of this kind. It is known from certain experience, and the conſequences thence deduced, that theſe maſſes have very little ſpecific gravity, and therefore muſt poſſeſs a very weak attractive power. According to every appearance, they ſeem to be compoſed of minute moleculæ mixed with the fine ethereal matter of light, and their trains are only highly ſubtile luminous and tranſparent ſubſtances; ſo that we have very little to apprehend from either of them.

No hiſtorian worthy of credit ever makes mention of the diſaſtrous effects of comets on our earth; and mankind are now too enlightened to be led into error by what the præjudice, ſuperſtition, and ignorance of former times have handed down to us on this ſubject.

Even in modern times comets have paſſed very near our earth, without producing any remarkable phænomena, either in regard to the atmoſphere or the common courſe of the weather; and much leſs were the poles or the axis of the earth any way affected by their approach; otherwiſe ſome traces of it would not have eſcaped the reſearches of the modern aſtronomers. On the other hand we know, by experience, that comets which have paſſed near to our earth, or the other planets, in their way to the ſun, have ſuffered ſome perturbation in their orbits by the attractive power of the latter.

I will not however aſſert that our earth, before it was completely formed and rendered habitable, and before every part of it was brought into complete equilibrium and ſtability, may not have experienced ſome changes and variations, in conſequence of which its poles and its axis may have been in ſome meaſure deranged; but all the traces of ſuch changes hitherto diſcovered in the thin cruſt within the reach of our obſervation ſeem to prove that they were produced by partial changes in the ſituation of certain parts of the land or ſea, which were in no manner able to diſplace the earth's centre of gravity, or to derange its poles and its axis.

In the courſe of a thouſand years, revolutions of this kind may take place in regard to the earth's ſurface, in conſequence of its organization, on account of the many chemical mixtures and ſolutions which are capable of continually exciting the active powers of nature; but the equilibrium, and powerful law of mutual attraction, by which one world is connected with another, may free us from all apprehenſion in regard to any derangement of the poles or axis of the earth capable of being followed with diſaſtrous conſequences.

LII. *Obſervations on the Manner in which the Spider* (Aranea Diadema) *ſpins its Web. By* M. C. G. LEHMANN *.

IT muſt be allowed by thoſe who have any knowledge of entomology, that the natural hiſtory of ſpiders has been much neglected; for, notwithſtanding the great progreſs which has been made in that ſcience, we are as yet very little acquainted with the organiſation, mode of life, and propagation of the moſt common kinds of theſe animals; and many ſpecies make a figure in our ſyſtems and cabinets which differ from each other only by their age or their ſex. Theſe inſects, ſo remarkable on account of their induſtry and manner of life, are viewed with a ſort of contempt and averſion, which would be inexplicable did we not know the great power of thoſe impreſſions and prejudices which we receive in our youth. Theſe in general are communicated to us by people who are the leaſt qualified to give the mind that direction neceſſary for the purpoſes of life, and who in particular ſeem to have united to inſpire us with a dread of ſpiders. Hence the complaint of ſo many naturaliſts, that this averſion always deters them from obſerving and accurately examining theſe inſects; and thoſe who have undertaken to do ſo have generally been obliged to take a great deal of trouble to overcome this antipathy *.

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* From *Der Geſellſchaft Naturforſchender Freunde zu Berlin Neue Schriften*, vol. iii. 1801.

But however much mankind may have beheld with averſion ſpiders and other vermin, they could not help admiring the ingenuity of the latter in ſpinning their webs, and the order and regularity diſplayed in theſe productions. As far back therefore as natural hiſtory can be traced, in the time of Ariſtotle, and in the fragments of his predeceſſor †, we find, if not accurate deſcriptions, at leaſt ſome mention of the labours of this inſect. In like manner, the moderns have thought the ſpider's web worthy of their attention; and it might conſequently have been expected from the accuracy of our naturaliſts, that they would have given us more minute and completer deſcriptions. This ought to have been the caſe in particular with the web of the croſs-ſpider, one of the moſt ſtriking and moſt remarkable. I have ſeveral times read ſuch deſcriptions, and ſtudied them with pleaſure, becauſe agreeable to nature; but always found this general complaint, that it is ſtill a ſecret in what manner the croſs-ſpider extends its firſt thread, in order that, when it has been faſtened, it may have a field for carrying on its work. It is well known that the croſs-ſpider diſpoſes its web in a more or leſs perpendicular direction in ſome open place. Thus, for example, we often find in a window four or five threads ſtretched from one ſide to the other; from theſe ſeveral radii proceed to a common centre; and around this centre, at a diſtance which may be eſtimated in general at a few lines, there are ſeveral concentric circles, each progreſſively larger; in the middle of which the ſpider places itſelf concealed, to wait until ſome inſect becomes entangled in theſe threads, when it immediately ruſhes forth and ſeizes its prey. Thus far the explanation of the proceſs which the ſpider employs is very eaſy, eſpecially as it is known that this inſect, wherever it goes, marks its way by a thread, which it leaves behind it; and that this thread, on account of its viſcoſity, can always be made faſt to any object, when the ſpi-

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* It is of importance to conſider by what means that averſion commonly called natural, and which is merely the reſult of improper education, can be overcome. Roſel accuſtomed himſelf to view theſe inſects firſt at a diſtance. He then conſidered their webs; and at laſt looked at the inſects themſelves through a microſcope. Göze firſt viewed individual parts of ſpiders, ſuch as the legs, head, &c. till he was at length able to look without any ſentiment of averſion at the entire inſect. Both theſe naturaliſts, by long habit, ſo far overcame this averſion, that they could handle and examine ſpiders with the ſame indifference as others can flies.

† Democritus. See Plin. Hiſt. Nat. lib. ii. cap. 24.

der only preſſes it a little, or moiſtens it with a drop of its viſcous liquid.

But theſe webs are commonly found in foreſts and woods, often on the high branches of different trees, not unfrequently ſuſpended between the roofs of different houſes ſtanding near each other, and even often extended over ſtreams and ponds of water, ſo that it is impoſſible to conceive how the ſpider could proceed from the one point to the other, in order to eſtabliſh her firſt thread, and to faſten to it the different radii. Some have endeavoured to account for this circumſtance by the moſt ſingular ſuppoſitions, as is commonly the caſe when philoſophers attempt to explain the phænomena of nature before they have examined them with ſufficient accuracy; but I ſhall not here trouble the reader with theſe hypotheſes, which contradict themſelves *. Accident, to which we are indebted for moſt of our diſcoveries, particularly in natural hiſtory, though we are unwilling to allow it any ſhare in our diſcoveries, gave me an opportunity of making an obſervation which decides the above queſtion. On a warm ſummer's evening I ſaw a croſs-ſpider *(aranea diadema)* let herſelf ſpeedily down from a branch of a tree by her uſual thread; and at the ſame time a multitude of ſimilar threads, which had their origin at the ſame place as the other, floated around; while the air, which was not perceptible to my ſenſes, carried them to a conſiderable diſtance, and even a conſiderable height. Though I approached with the utmoſt gentleneſs, the direction of the floating threads was immediately changed, and in a moment they had reached a neighbouring branch, to which they adhered. The ſpider ſoon obſerved that her floating threads had reached a fixed point, and immediately clambered up to them, having thus given me an opportunity of ſeeing the ſecret of the commencement of her ingenious labour. Guided by this trace, it was now eaſy for me to make my obſervations with greater accuracy; and the reſult of them is as follows. When a ſpider is deſirous of proceeding to a diſtant place, ſhe ſuddenly lets herſelf down by a pretty ſtrong thread, and at the ſame time draws one of her hind legs over the glandular parts which contain the materials of her web. By theſe means ſhe forces from them a great number of ſmall threads, and conſigns them to the air; which, on account of their ſpecific lightneſs, makes them float, ſo that they remain at that height at which the ſpider firſt throws them out. As ſhe then ſuddenly ſinks down in a retrograde direction, the firſt ones are extended to a conſiderable length.

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* I ſhall only mention one.—Liſter in his *Hiſt. Animal. Angliæ*, which contains a great many important obſervations on ſpiders, explains this, p. 7. as an *ejaculatio* filorum, and endeavours to make it more evident, by comparing it to the manner in which the ſun emits his rays.

But if the leaſt breath of air then takes place, they are thereby carried to a conſiderable diſtance, and ſtill become longer, as the inſect can then ſpin them out at pleaſure from her receptacles; ſo that they ſoon find ſome object to which, on account of their viſcoſity, they adhere. The ſpider then clambers up to them, ſtill keeping herſelf in connection with her firſt ſtation, as ſhe always carries with her the thread by which ſhe firſt let herſelf down, and faſtens the ſecond end of it when ſhe has reached a new point of reſt.

I have too little acquaintance with works on natural hiſtory to be able to determine whether this obſervation has before been made by others [*]; but by ſome queſtions in the Hanoverian Magazine, and by the private information of diſtinguiſhed naturaliſts, it appears that it is not generally known. But whether it be new, or may only ſerve to confirm what has been already remarked, I am happy that I have it in my power to point out to other ſearchers into nature how they may at all times be convinced of the truth of what I have here ſtated. If a perſon takes a ſpider from its web, and places it on the hand, it will endeavour to eſcape from this warm object as a place of danger, and let itſelf down in the manner above deſcribed; but if one of the threads that float about in the air be laid hold of by the other hand, the ſpider ſuffers herſelf to be drawn over by it; and this experiment can be repeated ſeveral times, till at length, ſeared by being ſo often deceived, ſhe throws herſelf looſe, and ſuffers herſelf to fall to the ground.

To theſe obſervations on the manner in which the ſpider begins her web, I ſhall add ſome others, which indeed comprehend but a ſmall part of the important diſcoveries that Quatremere Disjonval pretends to have made; and reſpecting which, ſince the attention of naturaliſts is called to this ſubject, I hope to obtain further information. The account that various kinds of ſpiders foretell changes of the weather ſeveral days before they take place, excited my whole attention; and I was the more deſirous of ſtudying the araneology of the above obſerver, as I was always of opinion that thoſe inſects which have organs peculiarly adapted for perceiving changes in the ſtate of the air [†] would be capable of giving us the beſt and earlieſt information reſpecting changes in the weather. It was impoſſible for me at firſt to procure Disjonval's work; and my curioſity began to be leſſened when I heard that no perſon was able to comprehend it. For this reaſon I reſolved to make obſervations myſelf; and with this view I collected a great many of the *aranea diadema* and *aranea domeſtica*, as the latter are thoſe which Disjonval muſt have had the beſt opportunity of obſerving when in priſon. The latter ſhowed no alteration in their web or their conduct, but in regard to the former I ſoon had an opportunity of making very important obſervations. I have already mentioned what every body knows, that this ſpider commonly ſits in the middle of its web. I now found that ſhe often extended a thread from the centre of her web, that is the uſual place of her reſidence, to the neareſt corner; in which ſhe concealed herſelf, inſtead of being in the web: and I always remarked, that this change was connected with a change of the weather, as all my croſs-ſpiders made the ſame change together; and immediately after the weather became very raw and cold. It is indeed very natural that thoſe inſects, when they have a preſenſation of bad weather, ſhould endeavour to ſhelter themſelves from it. But my obſervations were too much interrupted by buſineſs and various circumſtances, and I was too ill provided with good inſtruments for comparing the ſtate of the weather and of the atmoſphere, ſuch as barometers, thermometers, hygrometers, &c. to be able to ſay any thing deciſive on the ſubject. What I have ſaid will however furniſh a hint to others who may be diſpoſed to carry theſe reſearches further.

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[*] Swammerdam, in his *Bib. Nat.* p. 24. has deſcribed a ſimilar obſervation; but his deſcription appears to me not very clear.

[†] See more on this ſubject in my *Commentatio de Senſibus externis Animalium exſ.* p. 37.

The ſame opportunity enabled me to make another obſervation, of which I do not recollect ever to have heard or read, viz. that the croſs-ſpider regularly deſtroys its web every twenty-four hours, and in the place of the old ſubſtitutes a new one. This it always does in the night-time, but ſuſpends the renovation of it when a cloudy ſtate of the weather affords it no hope of catching any prey, and perhaps when any mechanical obſtacles ſtand in the way[*]. This obſervation has thrown great light upon many things relating to the natural hiſtory of ſpiders. It points out the principal method which ought to be purſued in making obſervations on the weather by means of theſe inſects, as they conſtruct no new web after deſtroying the old, when impeded by the unfavourable ſtate of the weather. This alſo enables us to explain why we often obſerve floating about, during very hot days, prodigious multitudes of ſpiders webs, commonly known under the name of *goſſamer*, and which has given occaſion to ſo much diſpute among philoſophers; for it may be readily conceived, when we reflect how numerous ſpiders are, what a multitude of theſe webs muſt be let looſe, when they all deſtroy their webs at the ſame time, in order to conſtruct new ones. During cloudy weather none of them are to be ſeen, for at ſuch periods none are let looſe; and even if there are a few of them in the atmoſphere, they acquire heavineſs from the moiſture they imbibe, of which they are very ſuſceptible, and of courſe fall to the earth. I was enabled alſo to determine the diſpute reſpecting the organs of viſion in ſpiders. They undoubtedly have eyes, which are indiſpenſably neceſſary for the functions they have to perform, and yet they do not ſeem to obſerve when a ſtick or other dangerous weapon is held cloſe to theſe organs, but they inſtantly retreat when in the leaſt touched. At preſent, however, this circumſtance can be eaſily explained, for theſe inſects are nocturnal animals. In the night-time they ſpin their webs, and during the morning and evening twilight catch the greateſt number of inſects. In the night they

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[*] That thoſe ſpiders which weave their web in the form of a wheel renew it daily, has been remarked by many obſervers as well as by myſelf. The cauſe may be eaſily conceived from what I have ſaid. For ſeveral years, during the ſummer, I reſided in a houſe ſituated in a garden; and oppoſite to the window, which was generally open, a large ſpider had extended her web. I took great care not to deſtroy it, becauſe it ſerved inſtead of one of thoſe gauze ſcreens which are commonly employed to keep out flies and gnats. The web in the day-time was frequently injured by accidents, but next morning I found it always repaired. One morning, having

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having got up earlier than uſual, I ſaw the ſpider at day-break deſtroy all the concentric threads proceeding from the centre outwards along the radii, breaking off with her feet the croſs threads, which ſhe cemented to the radii ſhe had left untouched; and which, by theſe means, became progreſſively thicker. She did the ſame with the principal threads by which the web was made faſt; and which, in conſequence of the threads added to them, were rendered much ſtronger. The ſmalleſt flies or gnats which attempted to enter through the window were therefore caught faſt in the net; but towards noon, or ſomewhat later, the flies became ſhyer, and no more were caught. The ſpider therefore about that time retired to a corner which ſhe had prepared in the window, where ſhe laid her eggs, and where ſhe remained quiet till the next morning. This obſervation ſhows that the threads of the web loſe their viſcoſity by the ſun's rays and the heat of the weather; and becoming too dry and hard to be fit for catching thoſe inſects on which the ſpider feeds, ſhe is under the neceſſity of renewing them. On the other hand, the principal threads, which ſerve only for ſtrengthening the web, have no need of being renewed. Damp weather may perhaps render theſe webs as unfit for catching inſects as the heat does, and therefore the ſpider thinks it unneceſſary to renew her web as long as that temperature continues.

ſee as well as the owls to catch their prey; and in the day-time, like thoſe birds, they are dazzled by the too great ſplendour of the light. But at that time they have no need of very acute ſight, as they ſeldom leave their web; and, when they do, are conducted back to it by their delicate ſenſe of touching. Beſides, they ſeldom have occaſion for it in the day-time, as their webs are then too viſible to the inſects which they catch; ſo that the latter can be better on their guard againſt them than they can during the twilight.

LIII. *Outlines of a View of Galvaniſm, chiefly extracted from a Courſe of Lectures on the Galvanic Phænomena, read at the Theatre of the Royal Inſtitution by* Mr. DAVY[*].

I. *Hiſtorical Introduction.*

§ 1. THE ſcience relating to the peculiar action of different conductors of electricity on each other, has lately excited a conſiderable degree of attention in the philoſophical world.

Owing its origin to the phænomenon diſcovered by Galvani[†], the production of muſcular contraction by the application of metals to the nerves and muſcles of animals, it has derived its name from that philoſopher.

Galvaniſm was at firſt limited in its application to organized bodies; but, in conſequence of the labours and inventive genius of experimentaliſts, our contemporaries, it has gradually become connected with chemiſtry and general phyſics; it has afforded powerful inſtruments of inveſtigation; and its operations have been traced throughout the whole of nature.

In giving an account of the progreſs of this ſcience, in its relation to the powers of the human mind, it will be ſufficient to notice ſuch experiments only as have derived their origin from extenſive theoretical views, and ſuch diſcoveries as have led to accurate generaliſations of phænomena already known.

Though the hiſtory of galvaniſm extends only through the period of the laſt nine years, yet we may notice in it four epochs, each of them diſtinguiſhed by the development of facts, variouſly intereſting from their novelty and the extent of their application.

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[*] From the *Journals of the Royal Inſtitution of Great Britain.*

[†] The firſt fact relating to the action of metals on the animal organs, was obſerved by Sulzer, who has deſcribed the ſenſation of taſte produced by the contact of lead and ſilver with the tongue, in his *Théorie des Plaiſirs*, publiſhed in 1767.

§ 2. Conſidering the firſt epoch as formed by the publication of the fundamental galvanic fact, we may derive the ſecond from the diſcovery of the exiſtence of inorganic galvaniſm. Till the reſearches of Fabroni, Dr. Aſh, and Creve, had been made known, the galvanic influence was generally conſidered as exiſting only in living animal organs. But the diſcovery of the peculiar action of metals in contact with each other upon water, demonſtrated the production of it in arrangements compoſed wholly of dead matter, and laid the foundation for a new claſs of inveſtigations, which have intimately connected the galvanic phænomena with known phyſical effects.

§ 3. The third epoch in the hiſtory of the ſcience is, perhaps, the moſt brilliant and moſt important. It will long be celebrated on account of the diſcovery of the accumulation of the galvanic influence. Before this diſcovery was made, the world, in general, beheld nothing deeply intereſting in galvaniſm: it had no relations to the common wants of life, and the facts that compoſed it were ſo obſcure as to be difficultly comprehended, except by long attention. The galvanic battery of Volta not only gratified the paſſion for novelty by the curious effects it produced, but likewiſe awakened the love of inveſtigation, by diſtinctly exhibiting the analogy between galvaniſm and common electricity.

§ 4. The fourth and laſt epoch in galvaniſm may be conſidered as founded upon the knowledge of the general connexion between the excitement of galvanic electricity, and chemical changes; and it chiefly owes its exiſtence to the labours of Britiſh experimentaliſts *. The diſcovery of the chemical agencies of galvaniſm has led to reſearches which finally cannot fail to elucidate the philoſophy of the imponderable or ethereal fluids. The year that is juſt paſt will long be diſtinguiſhed in the hiſtory of ſcience; ſeldom has phyſical inveſtigation been purſued with greater ardour; and if new facts, by being ſometimes inſulated and incapable of application to eſtabliſhed theories, have perplexed the public mind, yet they have at the ſame time been uſeful to it, by producing a habit of rational and active ſcepticiſm, which cannot fail of becoming, at a future period, the parent of truth.

II. *Of the leaſt complicated Galvanic Arrangements*, i. e. *ſimple Circles.*

§ 1. The conductors of electricity, which, by their action on each other, are capable of producing galvanic effects, may be divided into two claſſes *. The one claſs compriſes what may be called perfect conductors, oxidable metallic ſubſtances and charcoal: the other includes leſs perfect conductors, which are either oxidated fluids, or ſubſtances containing theſe fluids.

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* Meſſrs. Nicholſon, Carliſle, Cruikſhank and Henry, Dr. Wollaſton, and Major Haldane.

The ſimpleſt galvanic arrangements require for their formation at leaſt two bodies of the ſame claſs and one of a different claſs *. With regard to the form of their aggregation, they muſt be ſo diſpoſed, that the bodies of the one claſs may be in contact with each other, in one or more points, at the ſame time that they are connected in other diſtinct points with the body of the other claſs.

§ 2. The ſimple galvanic circles may be divided into two general kinds.

The firſt is formed by two different metallic ſubſtances, or one metallic ſubſtance and charcoal, and a peculiar fluid.

The ſecond is compoſed by two different fluids and one metallic ſubſtance.

Thus, if plates of zinc and of ſilver be made to touch in one point, and be connected together in other points by a portion of common water or of muriatic acid, a galvanic ſimple circle is formed of the firſt order.

Or if ſeparate portions of nitric acid and of water, moiſtening pieces of cloth or bibulous paper, be brought in contact with each other on a ſmall ſurface, at the ſame time that other ſurfaces of them are connected with different parts of a plate of tin, a circle of the ſecond kind is compoſed †.

§ 3. All arrangements, however, of two conductors of one claſs with one of the other, are not capable of producing galvanic effects: and even the powers of acting circles are very different in degree. It appears from all the facts, that chemical changes taking place in ſome of the parts of the circle are intimately connected with its agencies. For, though a momentary circulation of galvanic influence may poſſibly be produced by the contact of three different bodies, yet it appears moſt likely that the permanent excitation of it depends upon a certain exertion of their chemical affinities.

The moſt powerful circles of the firſt kind are thoſe compoſed of two ſolids of different degrees of oxidability, and of a fluid capable of oxidating at leaſt one of the ſolids ‡. And, even in the feebleſt circles, it appears that ſome chemical action is uniformly exerted either by oxidating fluids or ſolutions of alkaline ſulphurets.

Thus ſilver and gold do not appear to evolve galvanic in-

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* Volta. † D. ‡ Ritter.

fluence when in contact with pure water, which is incapable of acting chemically upon either of the metals; though when they are connected with water, holding in ſolution nitric acid, or any other fluid decompoſable by ſilver, they form an active galvanic arrangement *.

And zinc and ſilver, which act very little with pure water, form a powerful combination with water holding in ſolution atmoſpheric air or acids †.

The following table of ſome circles of the firſt kind, in which the different ſubſtances are arranged according to the order of their known galvanic powers, will ſhow how intimately chemical agencies are related to the production of galvaniſm.

TABLE OF SOME GALVANIC CIRCLES,

Compoſed of two perfect Conductors and one imperfect Conductor.

More oxidable ſubſtances.	Leſs oxidable ſubſtances.	Oxidating fluids.
Zinc	With gold, charcoal, ſilver, copper, tin, iron, mercury.	Solutions of nitric acid in water, of muriatic acid and ſulphuric acid, &c. Water holding in ſolution oxygen, atmoſpheric air, &c.
Iron	. . . . gold, charcoal, ſilver, copper, tin.	
Tin	. . . . gold, ſilver, charcoal.	
Lead	. . . . gold, ſilver.	
Copper	. . . . gold, ſilver.	Solution of nitrate of ſilver and mercury. Nitric acid, acetous acid.
Silver	. . . . gold.	Nitric acid ‡.

The moſt active ſingle circles of the ſecond order are thoſe in which the two imperfect conductors are capable of exerting different chemical agencies on the perfect conductor, at the ſame time that they are poſſeſſed of power of action on each other. But even circles in which only one of the fluid parts is decompoſable by the ſolid, are poſſeſſed of power of action.

Thus copper, ſilver, or lead, acts very powerfully when connected in the proper order with ſolutions of alkaline ſulphurets and of nitrous acid, both of which fluids are poſſeſſed of diſtinct chemical agencies upon them §. And copper or ſilver acts, though with leſs intenſity, when water, or a fluid which they are incapable of decompoſing, is ſubſtituted for one of the chemical agents.

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* D. † Fabroni.

‡ Dry nitre, cauſtic potaſh, and ſoda, are conductors of galvaniſm when rendered fluid by a high degree of heat; but the order of their conducting powers has not been yet aſcertained.

§ D.

The following table contains ſome powerful galvanic combinations of the ſecond order, arranged according to the intenſity of their action.

TABLE OF SOME GALVANIC CIRCLES,

Compoſed of two imperfect Conductors and one perfect Conductor.

Perfect Conductors.	Imperfect Conductors.	Imperfect Conductors.
Copper Silver Lead Tin Iron Zinc	Solutions of alkaline ſulphurets, capable of acting on the firſt three metals, but not on the laſt three.	Solutions of nitrous acid, oxygenated muriatic acid, &c. capable of acting on all the metals.

§ 4. Arrangements, exactly ſimilar in their action to the common ſimple circles, may be formed by the combination of more than three conductors. For, that ſimple galvanic action may be evolved, nothing more is apparently requiſite, than that the points of contact between the ſolid and fluid parts of the circle, *i. e.* the places where chemical affinities are liable to be exerted, be immediately preſerved; at the ſame time that thoſe parts are reſpectively homogeneal, or compoſed reſpectively of ſimilar conductors. Thus zinc, ſilver, iron, gold, and water, arranged in a circle, in the order of their enumeration, produce action exactly analogous to zinc, gold, and water. And nitrous acid, water, ſolution of cauſtic potaſh, ſolution of ſulphuret of potaſh, and ſilver, evolve galvanic influence in the ſame manner as nitrous acid, water, ſolution of ſulphuret of potaſh, and ſilver.

It appears, however, that the length and ſurface of the conducting ſeries, connecting the exciting parts of the circle, are in ſome meaſure related to the quantity of action it is capable of exhibiting. This law, indeed, applies only analogically to perfect conductors; though, with regard to imperfect conductors, it is demonſtrated experimentally; as in all caſes a diminution of power, in ſome unknown ratio, is produced by the increaſe of the length, or, what is apparently equivalent, the diminution of the ſurface of the chain which they compoſe.

The limits of ſurface, and of length, of imperfect conductors in circles, connected with the maximum and minimum of their activity, have not been yet aſcertained; and there is every reaſon to believe that they will be different in different imperfect conductors. Thus, portions of ſulphuric acid and of nitrous acid will probably form better conducting ſeries than ſimilar portions of water; and a chain of water will perhaps act better than an equal chain of water mixed with alcohol.

From ſeveral experiments, it would appear that the powers of circles are particularly connected with the extenſion of the ſurfaces of contact between the perfect and imperfect conductors.

§ 5. All galvanic influence in ſingle circles is manifeſted either by its efficiency in affecting living animal organs, forming part of the arrangement; or by its power in modifying the chemical changes produced by the action of the perfect on the imperfect conductors.

A. When one fluid part of a powerful ſingle circle is made to touch the tongue at the ſame time that another fluid part is connected with ſome irritable ſurface of the body, an acid taſte is perceived*, which becomes leſs diſtinct in proportion as the contact is longer preſerved.

If the eye be made part of the galvanic circuit, a tranſient flaſh of light is produced at the moment the chain is completed †; and, when the bared cutis is employed, a faint painful ſenſation uniformly denotes the circulation of the galvanic influence ‡.

But the moſt diſtinct mode of exhibiting, by animal excitement, the exiſtence of ſmall quantities of galvanic power, is by connecting one part of the circle with a nerve, and another part with a muſcle, in a limb juſt ſeparated from the body of a living animal. In this caſe, whenever the connection of the arrangement is made or broken, muſcular contractions are produced §.

B. In galvanic ſimple circles all primary chemical action taking place between the imperfect and perfect conductors is apparently increaſed by their galvanic arrangement. Thus zinc, which oxidates when in contact with common water alone, oxidates much more rapidly when forming a galvanic circle with it by means of gold, or other difficultly oxidable metals ‖. And tin appears to diſſolve faſter in diluted nitric acid, when compoſing a circle with it by the contact of gold, than when ſingly immerſed in it ¶.

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* Sulzer, Volta, Robiſon, and other philoſophers.

† Mr. Hunter. ‡ Humboldt. § Galvani.

‖ Dr. Aſh. ¶ Original experiment, D.

C. But increaſe of primary chemical action is not the only inorganic effect of galvaniſm; for it changes the nature of this action in a very peculiar manner. In the oxidating circle with common water, no perceptible quantity of hydrogen is evolved, as in common proceſſes; but an alkaline ſubſtance appears to be formed at the point of contact of the leaſt oxidable perfect conductor with the fluid. Thus, if zinc and ſilver be made to form a circle with diſtilled water, holding in ſolution air, for many weeks, a conſiderable oxidation of the zinc is perceived, without the perceptible evolution of gas; and the water, at its point of contact with the ſilver, becomes poſſeſſed of the power of tingeing green, red cabbage juice, and of rendering turbid, ſolution of muriate of magneſia *.

In the oxidating circles with acids, gas is not only evolved from the parts of the conductors undergoing chemical change, but likewiſe from other parts in which no primary action apparently exiſts. Thus, when zinc and ſilver form a circle with diluted muriatic acid, gas is not only given out at the points of contact of the acid with the zinc, but likewiſe at the place where it is connected with the ſilver †. And in the circle with tin, gold, and diluted nitric acid, nitrous gas is evolved from the gold as well as from the tin *.

D. Indeed in all the ſingle galvanic circles, whenever an oxidating influence is exerted at one of the places of contact of the perfect and imperfect conductors, a deoxidating action appears to be produced at the other place. Thus, when iron, which oxidates rapidly when forming a circle with ſilver and common water, is arranged with zinc and common water, it remains perfectly unaltered, whilſt the zinc is rapidly acted upon.

§ 6. The phænomena exhibited by the ſimple galvanic circles cannot well be theorized upon, except in the laws of their connection with the more complicated and more ſtriking galvanic facts. And it is from the diſcovery made by Volta, of the accumulation of galvaniſm only, that the inſtruments of inveſtigation are derived, by means of which the nature of this influence is known, and its moſt important agencies and relations aſcertained.

III. *Of Compound Galvanic Circles, or Galvanic Batteries.*

§ 1. The inſtruments for accumulating galvanic power, or the compound galvanic circles, are compoſed of the ſame elements as the ſimple circles; but thoſe elements are diſpoſed in a different form of aggregation.

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* Original experiment, D.

† Dr. Wollaſton.

To compoſe a galvanic battery, ſeries of the conductors capable of forming ſimple circles are required. And they muſt be arranged in ſuch a manner, that the conductors of the ſame claſs in every ſeries may be in contact with each other, in one or more points, at the ſame time that they are reſpectively connected with different conductors of the other claſs, and one of them belonging to the ſame ſeries, a regular alternation being obſerved.

Thus, the order of a compound galvanic circle is, conductor of the one claſs, conductor of the one claſs, conductor of the other claſs; conductor of the one claſs, and ſo on, in uniform arrangement.

§ 2. The compound galvanic circles, like the ſimple circles, may be divided into two general orders.

The firſt order is compoſed of ſeries, containing, at leaſt, two perfect conductors, and one imperfect conductor. The ſecond is formed by arrangements, conſiſting of not leſs than two imperfect conductors, and one perfect conductor[*].

Thus, if plates of zinc, and of ſilver, and pieces of cloth, of the ſize of the plates, moiſtened in water or diluted muriatic acid, be arranged in the order of zinc, ſilver, cloth; zinc, ſilver, cloth; and ſo on, till twenty ſeries are perfectly connected, a galvanic battery is formed of the firſt claſs[†].

And if plates of copper, and pieces of cloth, moiſtened, ſome in water and ſome in ſolution of ſulphuret of potaſh, be connected in a circle, in the order of copper, cloth moiſtened in water, cloth moiſtened in ſolution of ſulphuret of potaſh, and ſo on, a compound circle will be formed of the ſecond claſs[‡].

The moſt elegant mode, however, of arranging the metals of compound circles with fluids, is by means of veſſels compoſed of electrical nonconductors, ſuch as glaſſes, or boxes formed of baked wood.

Thus, plates of copper and of zinc, paired, and ſoldered together at their edges, may be cemented into a trough compoſed of baked wood, in regular alternation, and in ſuch a manner as to leave a number of water-tight cells correſponding to the number of ſeries. And this arrangement will become active when the cells are filled with water or ſaline ſolutions, and when the extreme metals are properly connected[§].

Or, inſtead of the double plates, ſingle plates of copper and of horn, or glaſs, may be uſed in uniform alternation; when the cells muſt be filled with different fluid chemical agents in the regular order, ſo as to be connected together by pieces of moiſtened cloth, paſſing over the nonconducting plates[*].

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[*] D.

[†] Volta.

[‡] D.

[§] Mr. Cruikſhank.

§ 3. The ſubſtances moſt active in the ſingle circles are likewiſe moſt active in the compound circles. And in all caſes, the relative quantities of galvanic power, exhibited by equal numbers of different ſeries, are, in ſome meaſure, proportional to the intenſity of the peculiar primary chemical agencies, exerted by the different conductors compoſing them, on each other[†].

Thus, in the battery with zinc and ſilver, there is little or no action produced, when the connecting fluid is pure water, or water holding in ſolution hydrogen gas, which is incapable of acting on the metals[†]. The action is greater with water ſaturated with oxygen[‡] than with water ſaturated with atmoſpheric air; and it is moſt intenſe when ſolution of red ſulphate of iron, or of nitric acid, is employed.

The tables of the ſingle circles will indicate, with the neceſſary change of arrangement, the relative powers of the ſeries forming compound circles.

§ 4. Provided thoſe places of contact in the compound circle, in which the peculiar chemical changes are produced, remain permanent, the parts of the ſeries which do not immediately act chemically on each other, may be connected together in the ſame manner as in ſingle circles, by means of conductors of their own claſs, without any alteration of the nature of the agencies.

Thus, in the circle with copper, iron, and water, the copper and iron may be connected by means of a wire of braſs[§]. And even the continuity of the portions of water may be broken by moiſt muſcular fibre, and other imperfect conductors, without any other change in the effect than a diminution of its intenſity[||].

§ 5. The galvanic influence, when highly accumulated, produces very extraordinary chemical and phyſical effects; and, in many of its appearances, aſſumes the preciſe form of common electricity.

A. When in a powerful battery (one, for inſtance, containing two hundred ſeries) the communication, after being broken, is again rendered complete by the contact of two perfect conductors, a flaſh or ſpark of light is perceived analogous to that produced by electricity[§]. This ſpark, or flaſh, when the battery is moſt powerful, is capable of paſſing through a conſiderable ſtratum of air[¶], and of inflaming mixtures of oxygen and hydrogen[**]. When the metallic ſubſtances, by which it is tranſmitted, are of very ſmall volume, it is poſſeſſed of the power of igniting them, and of making them enter into combuſtion when in contact with oxygen [*].

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[*] Count Rumford.
[†] D.
[‡] Major Haldane.
[§] Volta.
[||] Pfaff, Ritter, and D.
[¶] Mr. Cruikſhank.
[**] Mr. R. Boulton.

B. The galvanic influence, when highly concentrated, affects the electrometer, and is capable of communicating weak charges to the condenſer and Leyden phial [†]. In all compound circles of the firſt claſs, the moſt oxidable part of the metallic plates evolves the influence, appearing as poſitive electricity, whilſt the leaſt oxidable part ſeems negatively electrified. In the ſecond claſs of circles it is, however, probable that this order is reverſed.

C. Galvaniſm, moderately accumulated, affects the animal organs in nearly the ſame manner as common electricity. When the human body is made part of the circle, a ſhock is perceived at the moment of connection; and a ſubſequent numbneſs and tingling ſenſation denote the permanent circulation of the influence.

The freſh limbs of frogs undergo violent contractions, and ſoon loſe their excitability, when placed in the circuit of a powerful battery.

D. The chemical actions taking place in the compound galvanic circle, preſent curious and moſt unexampled appearances; all the primary changes taking place in the different conductors being increaſed, and modified to a great extent.

In all batteries of the firſt order, when the connection is completed, changes take place which denote the evolution of influences capable of producing from *common* water oxygen and hydrogen, acid and alkali, in different parts of every ſeries [‡].

Thus, in the battery with ſeries of zinc plates, ſilver wires, and common water, oxide of zinc is formed on all the plates of zinc, whilſt hydrogen is produced from the ſilver wires, and if the water in contact with them be tinged with red cabbage juice, it becomes green [§].

And in the battery with ſilver, gold, and weak nitric acid, the ſilver is diſſolved, whilſt the acid becomes green, and ſlowly evolves gas at its points of contact with the gold.

The chemical agencies exerted in the compound batteries of the firſt claſs, can be beſt obſerved by the ſubſtitution of ſingle metallic wires for ſome of the double plates; for, in this caſe, the changes taking place in the ſeries with wires, will be exactly analogous to thoſe produced in the ſeries with plates; ſilver, and all the more oxidable metals, oxidating in water, in the uſual place; and gold and platina evolving oxygen gas.

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[*] Profeſſors Tromsdorf Böckmann, Fourcroy, and Vauquelin.

[†] Mr. Nicholſon and Mr. Cruikſhank.

[‡] Mr. Nicholſon, Mr. Carliſle, and Mr. Cruikſhank.

[§] D.

Thus, when into two ſmall glaſs tubes, connected by moiſt animal ſubſtance, and filled with diſtilled water, two gold wires are introduced from a large battery, in the proper order, oxygen is produced in one quantity of water, and hydrogen in the other, nearly in the proportions in which they are required to form water by combuſtion *. And if the proceſs be continued for ſome time, the apparatus being expoſed to the atmoſphere, the water in the oxygen-giving tube will become impregnated with an acid (apparently the nitrous); whilſt that in the hydrogen-giving tube will be found to hold in ſolution an alkali, which, in certain caſes, has appeared to be fixed †.

From ſome experiments it would appear probable that the quantities of hydrogen produced in ſeries are ſmall, and the quantities of alkali great, in proportion as the ſurfaces of contact of the leaſt oxidable metals with the water are more extended *.

All the oxygenated ſolutions of bodies poſſeſſing leſs affinity for oxygen than naſcent hydrogen, are decompoſed when expoſed to the action of the metal occupying the place of the leaſt oxidable part of a ſeries in the compound circle.

Thus, ſulphur may be produced from ſulphuric acid; and copper and other metals precipitated in the metallic form from their ſolvents ‡.

But little knowledge has yet been obtained concerning the chemical changes taking place in the ſecond claſs of galvanic batteries. But from ſeveral experiments it would appear that they are materially different in the laws of their production from thoſe taking place in the firſt claſs.

Thus, when ſingle metallic wires with water are placed as ſeries in powerful batteries of the ſecond order, the influence producing oxygen ſeems to be tranſmitted by the point, in the place of that part of the plate, which was apparently incapable of undergoing oxidation; whilſt the hydrogen is evolved from that point, where the oxidating part of the primary ſeries appeared to exiſt §.

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* Original experiment, D.

† Mr. Cruikſhank, the firſt diſcoverer of the galvanic production of alkali, ſuppoſes that the hydrogen wire in common water generates ammoniac. Perhaps the preſence of muſcular fibre is connected with the production of fixed alkali in the experiment detailed in the text.

‡ Mr. Nicholſon, Mr. Cruikſhank, and Mr. Henry jun.

§ D.

§ 6.

§ 6. The agency of the galvanic influence, which occaſions chemical changes, and communicates electrical charges, is probably, in ſome meaſure, diſtinct from that agency which produces ſparks, and the combuſtion of bodies.

The one appears (all other circumſtances being ſimilar) to have little relation to ſurface in compound circles, but to be great, in ſome unknown proportion, as the number of ſeries are numerous. The intenſity of the other ſeems to be as much connected with the extenſion of the ſurfaces of the ſeries, as with their number.

Thus, though eight ſeries, compoſed of plates of zinc and copper, about ten inches ſquare, and of cloths of the ſame ſize, moiſtened in diluted muriatic acid, give ſparks ſo vivid as to burn iron wire; yet the ſhocks they produce are hardly ſenſible, and the chemical changes indiſtinct *. Whilſt twenty-four ſeries of ſimilar plates and cloths, about two inches ſquare, which occaſion ſhocks and chemical agencies more than three times as intenſe, produce no light whatever.

§ 7. A meaſure of the intenſity of the power in galvanic batteries, producing chemical changes, may be derived from the quantity of gas it is capable of evolving from water in a given time; or from the length of the fluid chain through which it can be tranſmitted. For the ſame law of diminution of activity that was applied to ſingle circles in § 4. p. 330, may be likewiſe applied to compound circles.

The general relative forces of different batteries may be aſcertained by connecting them in oppoſite orders, ſo as to produce a certain annihilation of power. For in all caſes when the moſt oxidable part of one ſeries is placed oppoſite to the ſame part of another equal ſeries, and connected with it by means of a fluid, the galvanic agencies of both are deſtroyed.

IV. *General Obſervations.*

§ 1. From a valuable experiment lately made †, it appears, that, when common electricity is paſſed through water, by means of two very fine metallic points, chemical changes are effected by it, analogous to thoſe occaſioned by the tranſmiſſion of the galvanic influence.

This fact, ſingly, preſents a very ſtrong analogy between galvaniſm and the common electrical influence; and, when compared with the other facts, it amounts almoſt to a demonſtration of their identity.

On this theory, it ſeems moſt probable, that all the different galvanic phænomena owe their exiſtence to electricity, excited in the different arrangements, in conſequence of a change in the electrical capacity of ſuch of their parts as undergo chemical action; this action being always connected with alterations in the ſurfaces and conducting powers of bodies.

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* Fourcroy, Vauquelin, and Thenard.

† By Dr. Wollaſton.

The circulation of electricity in galvanic circles, from its different points of excitation, appears to depend, in a great meaſure, upon certain ſpecific attractions for it, in the different conductors, inexplicable by known laws.

§ 2. In all caſes of ſimple galvanic action, negative and poſitive electricity, as reſpectively exhibited by their deoxidating and oxidating influences, after being once excited, can be multiplied by a certain alternation of ſurface.

Thus, in the circle with zinc, iron, and water, in which, as was ſtated in page 332, the zinc oxidates, and the iron undergoes no change, if a drop of water be placed on an inſulated ſurface of the iron, it will rapidly act upon it, and produce red oxide; whereas, if it be placed upon a ſimilar ſurface of the zinc, it will undergo very little change *.

This fact will, perhaps, in ſome meaſure, lead to an explanation of the cauſe of the accumulation of galvaniſm in compound circles. For, in thoſe circles, all the ſeries are ſo conſtituted as to produce a perfect correſpondence between the electricity excited by chemical action, and that produced by compenſation.

§ 3. The general chemical agency of galvaniſm is at preſent involved in obſcurity. The facts relating to the ſeparate production of oxygen and hydrogen, acid and alkali, in water, are totally incommenſurable with the uſually received theory of chemiſtry. And, even ſuppoſing that the appearance of the two laſt of theſe bodies is connected with the preſence of atmoſpheric air, it is ſtill extremely difficult to conceive, that either oxygen or hydrogen can paſs in an inviſible form through fluids or organic bodies. Common phyſical facts preſent us with no analogies ſufficiently diſtinct to enable us to reaſon on this ſubject; and the elucidation of it will probably be connected with perfectly new views of corpuſcular action.

§ 4. The appearance of galvanic action in living matter, particularly in the torpedinal organ, leads to curious inquiries. Chemical changes are perpetually going on in different parts of the living body, which muſt be connected with alterations in their ſtates of electricity; and organized beings contain all the ſubſtances requiſite for forming ſpecies of galvanic arrangements These circumſtances, combined with the facts

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* D.

of the production of muſcular contraction by common galvaniſm, and the dependance of irritability, and even life, upon the oxygenation of the blood, afford analogies which render it probable that ſome phænomena ſimilar to the galvanic phænomena may be connected with muſcular action and other proceſſes of life. Theſe analogies, however, at preſent are very indiſtinct; and they ought to be conſidered of importance only ſo far as they are likely to lead to the diſcovery of new inſtruments of experimental inveſtigation.

Concluſion.

The relations of galvaniſm to the different branches of phyſical ſcience are too numerous and too extenſive to be connected with the preceding details; and, although in their infancy, they will probably long conſtitute favourite ſubjects of inveſtigation amongſt philoſophers, becoming the ſources of uſeful diſcoveries.

The new galvanic facts have given an importance to the ſcience ſufficient to render it intereſting, and to enſure its progreſſion. The ſpirit of inquiry is awakened in the public mind, and it is difficult to imagine the exiſtence of cauſes capable of deſtroying it.

Galvaniſm may be purſued with greater facility than moſt of the other ſciences; it requires leſs time and attention; it is leſs connected with manual labour, and the moſt delicate organs are the beſt fitted for performing and obſerving its operations.

The inſtruments required for galvanic experiments are few, and but little expenſive. A battery compoſed of fifty plates, when arranged with chemical agents, forms a combination ſufficiently powerful for common experiments. With ſuch a combination, a few glaſs tubes having gold wires attached to them, and a gold leaf electrometer, may inveſtigations be purſued, and principles diſcovered, extending not alone to the laws of dead matter, but even to thoſe of animated nature.

If, to render galvaniſm a popular ſtudy, it were neceſſary to prove that it bore relations to the common wants of life, it might be ſtated, that its agencies are likely, at ſome period, to become uſeful in the arts. When our galvanic inſtruments are rendered more perfect and more powerful, we may be readily enabled, by means of them, to procure the pure metals; and to form immediately from their elements, nitrous acid and alkali. The connection of galvaniſm with philoſophical medicine is evident. The electrical influence in its common form, as excited by machines, has been employed with advantage in the cure of diſeaſes; in a new ſtate of exiſtence it may poſſibly be poſſeſſed of greater, and of different powers.

But, independent of the immediate applications of this ſcience, much is to be hoped from the elucidations which it may beſtow upon the kindred ſciences. And a diſcovery ſo important as to excite our aſtoniſhment, cannot fail of becoming, at ſome period, uſeful to ſociety. All the different branches of human knowledge are intimately connected together, and theoretical improvements cannot well be made in them without being accompanied by practical advantages.

Royal Inſtitution, Sept. 1, 1801.

LIV. *An Account of a Method of conſtructing Simple and Compound Galvanic Combinations, without the Uſe of Metallic Subſtances, by means of Charcoal and different Fluids.*

By Mr. DAVY *.

1. IF a piece of well burned charcoal be brought in contact at one of its ſurfaces with a portion of water, and at another ſurface with a portion of nitric acid, a ſimple galvanic combination will be formed when the two fluids are connected together: and the powers of it are demonſtrated by its agencies upon the limbs of frogs, and by its effects upon the organs of ſenſe.

2. A compound galvanic combination or a galvanic battery may be formed from a number of ſeries compoſed of the ſame ſubſtances: but in this caſe the fluid elements of each ſeries, not being immediately in contact, muſt be connected with ſimilar elements in other ſeries in an order of regular alternation, ſuch as water, charcoal, acid; water, charcoal, acid; and ſo on.

3. The beſt mode that has yet occurred of conſtructing galvanic batteries with charcoal, is by means of a number of glaſſes, which are made to contain, alternately, nitrous acid and water, and which are connected in pairs by means of moiſtened cloth. The pieces of charcoal uſed are made from very denſe wood, ſuch as box or lignum vitæ; and in this caſe, the fluids will not penetrate into them by capillary attraction much beyond the places of their primary contact. Their forms are thoſe of arcs, ſo that each piece connects together two glaſſes; but in inſtances where ſingle pieces of charcoal cannot be obtained of the proper ſhape, two long and thin ſlips may be faſtened together by ſilk, ſo as to form the angle neceſſary to their inſertion into the glaſſes.

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* From the ſame.

4. Twenty ſeries in a battery of this kind produce ſenſible but feeble ſhocks, and when a ſingle metallic ſeries with a gold wire and two glaſſes of water is ſubſtituted for one of the primary ſeries, hydrogen is given out by the metallic point in the glaſs of water in the place of the acid, whilſt oxygen is evolved from the point in the other glaſs.

5. In the galvanic batteries with charcoal, ſulphuric acid may be ſubſtituted for nitric acid; and ſolution of ſulphuret of potaſh for the water, without any material alteration in the nature of the agency; the ſolution of the ſulphuret indeed, ſeems, in ſome meaſure, to increaſe its intenſity, and combinations containing this ſubſtance, denſe charcoal, and concentrated nitric acid, appear to be ſuperior in activity to ſimilar combinations containing copper, and the ſame fluid elements, and to be nearly equal to thoſe compoſed of zinc, ſilver, and water.

January 9, 1802.

LV. *Obſervations on the Affinity which Earths have for each other.* *By* DARRACQ, *Pupil of* VAUQUELIN, *and his Aſſiſtant in the School of Mines*[*].

THE analyſis of ſtones has long engaged the attention of chemiſts. The moderns, aſſiſted by more correct inſtruments, have carried this branch of chemiſtry to ſuch a degree of perfection, as ſeems to leave nothing more to be deſired; but C. Guyton, in a memoir, in which he treats with minuteneſs on the different affinities which the earths exerciſe on each other, relates ſeveral obſervations, printed in the 27th number of the *Annales de Chimie*, which excite great doubt in regard to the exactneſs of the analyſes hitherto made, and which invite all chemiſts to be on their guard, leſt they ſhould take a ſimple earth for a mixture of ſeveral. The embarraſſment which theſe affinities daily gave riſe to, determined me to examine, with great care, the different circumſtances under which they may take place; for, if it is of importance to point out theſe new combinations, it is no leſs ſo to reduce them to their juſt value.

Exp. I. C. Guyton having mixed ten centilitres of lime water with two centilitres of barytes water, there were ſoon formed white clouds, which united, and depoſited themſelves at the bottom of the veſſel containing the mixture. In conſequence of this experiment he admitted an affinity between lime and barytes.

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[*] From the *Annales de Chimie*, No. 118.

I repeated the ſame experiment with great care; but obtained no change in the liquors mixed, nor any ſign of precipitation, even after five months. It is to be preſumed that the precipitate formed in the experiment of C. Guyton aroſe from the impurity of the ſubſtances employed. In my opinion, it was owing to the preſence of ſome atoms of ſulphuric acid contained in the lime. It is almoſt always found in common lime.

Exp. II. By combining a ſolution of aluminated potaſh and ſiliciated potaſh, I obtained, as Guyton did, a very thick white precipitate, which ſoon after formed a gelly. This precipitate, diſperſed in water, and filtered, gave, by the teſts proper for alumine and ſilex, evident proofs of the affinity of theſe two earths diſſolved in potaſh.

Exp. III. Lime water, mixed with a ſolution of ſiliciated potaſh, gave alſo proofs of a real affinity between theſe earths by the precipitation, which was inſtantly effected. This was confirmed alſo by an analyſis of the precipitate.

Exp. IV. Strontian water with ſiliciated potaſh produced the ſame phænomenon as the preceding mixture.

Exp. V. Strontian water and lime water mixed together formed no precipitate, as was obſerved by Guyton.

Exp. VI. Strontian water and barytes water, when combined, formed no precipitate.

Exp. VII. Carbonates of lime and barytes, diſſolved in an exceſs of their acid, gave no ſign of a new combination.

Exp. VIII. This experiment, which begins the ſeries of thoſe made by Guyton on the ſolution of earths in acids, did not afford me the reſults announced by that chemiſt. Having made a mixture of equal parts of a ſolution of muriate of lime and a ſolution of muriate of alumine, he obſerved, 1ſt, That the liquor immediately became turbid, without the acid of agitation: 2d, That ſoon after it was opake, and almoſt gelatinous: 3d, and in the laſt place, that this precipitate did not diſappear by the addition of an acid.

I repeated this experiment, with all the care poſſible, to obtain the ſame phænomena. Very pure muriate of lime, mixed with muriate of alumine equally pure, gave no ſign of precipitation: the mixed liquors conſtantly remained clear.

It is probable, according to ſome eſſays which are peculiar to myſelf, that the precipitate obtained by C. Guyton was occaſioned by the ſulphuric acid contained in the muriate of alumine, and which at the time of the mixture muſt have re-acted on the lime; for when alumine has been extracted from alum, it is difficult to free it entirely from ſulphuric acid.

By extracting alumine from thoſe natural ſtones which contain it, either by the nitric acid or the muriatic acid, it will be eaſy to diſcover the error into which the author was led by muriate of lime prepared with the earth of alum.

Exp. IX. By combining muriate of lime, in ſolution, with muriate of barytes, Guyton obſerved an abundant precipitate, which was not diſſolved by exceſs of muriatic acid. The ſolutions of theſe two muriates, whether concentrated, or diluted with diſtilled water, gave no ſign of precipitation by mixing together, nor a long time after their combination. In my opinion, the muriate of lime employed by Guyton contained ſome atoms of ſulphuric acid, which, with the barytes, muſt have formed ſulphate of barytes.

Exp. X. Muriate of lime mixed with that of magneſia gave, as Guyton obſerved, no ſign of precipitation.

Exp. XI. When ſolutions of muriates of lime and of ſtrontian were mixed, no change was produced. This was obſerved alſo by Guyton.

Exp. XII. Guyton announces, that the mixture of ſolutions of muriate of magneſia and muriate of alumine aſſumed, at the end of ſome minutes, a ſlight milky appearance. This experiment, being carefully repeated, exhibited nothing of the kind: my liquors retained their clearneſs and tranſparency.

Exp. XIII. The thirteenth experiment, made with a mixture of ſolutions of muriate of magneſia and muriate of barytes, formed no precipitate, either at the moment or long after; yet Guyton ſays he obtained an abundant precipitate. It is probable that ſulphuric acid was ſtill concealed in the muriate of magneſia.

Exp. XIV. This experiment, which was made with muriate of magneſia and muriate of ſtrontian, gave no ſign of any new combination. The ſame was remarked by Guyton.

Exp. XV. Muriate of barytes and muriate of alumine, both diſſolved and even diluted in water, gave to Guyton an abundant precipitate. The ſame experiment, repeated with the ſolutions concentrated or diluted with water, did not yield me the ſame reſult: the mixture always remained clear, without forming any precipitate.

Exp. XVI. The ſixteenth experiment, made by the combination of muriate of ſtrontian with that of barytes, exhibited no change.

Exp. XVII. The ſeventeenth and laſt experiment of Guyton, on the affinity of earths in the humid way, was made by mixing a ſolution of muriate of ſtrontian with a ſolution of muriate of alumine. According to his account, the liquor aſſumed a milky colour, and gave a precipitate which the acid did not rediſſolve. This experiment, repeated, like all the reſt, with a great deal of care, ſtill confirmed the preſence of the ſulphuric acid in the ſolution of muriate of ammonia he employed; for I had no precipitate when I repeated the experiment, and the liquor, for five months, loſt nothing of its tranſparency.

By admitting between earthy ſubſtances an affinity of which nature affords a thouſand examples, I do not however think that the force which attracts them is ſufficiently great to make them abandon their ſolvent, without evaporation, eſpecially if it be acid. There is none indeed but ſilex diſſolved in alkali that has the property of precipitating the other alkaline earths from their aqueous ſolutions: the other earths produce nothing of the kind; for, if alumine diſſolved in an alkali be mixed with ſolutions of barytes, ſtrontian, lime, &c. no precipitate will be formed; nothing more will take place between the ſtrontian, lime, barytes, &c.

If the experiments of Guyton had really eſtabliſhed the facts he announces, we ſhould have had no longer any certain means for analyſing earths and ſtones, and every thing hitherto done on this ſubject could only have been conſidered as ſo many incorrect reſults.

I therefore think myſelf authorized to conclude, from the facts announced in this memoir, that the phænomena obſerved by Guyton were occaſioned in many caſes by foreign bodies contained in the matters he employed.

LVI. *Experiments concerning the Analyſis and Syntheſis of Alkalies and Earths, announced by* C. GUYTON *and* DESORMES. *By* C. DARRACQ, *Pupil of and Aſſiſtant to* C. VAUQUELIN *at the School of Mines*[*].

THE third volume of the Memoirs of the Inſtitute of France contains ſome experiments by C. Guyton and Desormes on the compoſition and decompoſition of the two fixed alkalies, and ſome of the earths ſuppoſed to be ſimple. The importance of ſuch diſcoveries, the period of their being made, and their utility to the arts, invite all chemiſts to repeat them; and ſuch were the motives by which I was guided in my reſearches. To announce that my experiments were made in the laboratory of the School of Mines, is to inform the public that I was aſſiſted by the advice of C. Vauquelin.

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[*] From the *Annales de Chimie*, No. 119.

I muſt requeſt thoſe chemiſts whoſe opinions I am about to oppoſe, to conſider my labour as ariſing only from a deſire to diſcover truth, and not as the reſult of a vain pretenſion to alter in any manner the eſteem which they deſerve, and the gratitude due to them for their numerous and uſeful diſcoveries. I ſhall therefore content myſelf with deſcribing the manner in which I repeated their experiments. I ſhall ſhow the reſults of them, and I ſhall leave to chemiſts to deduce from them ſuch concluſions as they may think proper.

Exp. I. The firſt experiment, which conſiſts in the reduction of a metallic oxide by potaſh, preſents nothing that can ſupport the obſervation of the above authors. The conſequence which they themſelves deduce from it is merely acceſſory. Beſides, it is known that ſeveral ſaline ſubſtances deſtitute of potaſh poſſeſs this property. It is known alſo, that fire alone is ſufficient to reduce the oxide of lead employed in this operation.

Exp. II. Guyton and Deſormes decompoſed in a platina crucible ſulphuret of potaſh; and with the help of a ſtrong heat they obtained lime.

I repeated this experiment with common ſulphuret of potaſh; that is to ſay, the ſubſtance commonly ſold in the ſhops under the name of *liver of ſulphur*; and having expoſed it to a ſtrong heat, I obtained a reſiduum which, when treated with water, was not entirely diſſolved. The inſoluble part, treated by nitric acid, was attacked; the nitric ſolution, tried by the oxalate of ammonia, gave a great quantity of lime.

Suſpecting that the lime found by this experiment might ariſe from the potaſh employed to make the ſulphuret, I made a ſecond experiment with pure ſulphuret of potaſh, prepared from potaſh purified by alcohol and ſublimated ſulphur. I employed 16 grammes of alkali and 8 of ſulphur. The mixture, made in a mortar, and poured by portions into a platina crucible ſtrongly heated, left a matter of a reddiſh brown colour, which, treated by water, was almoſt entirely diſſolved. The ſmall reſiduum, which weighed only 4 centigrammes, being carefully examined, was found to be ſilex coloured by charcoal; it aroſe no doubt from the potaſh, which, as is well known, eaſily diſſolves that earth.

Exp. III. To perform this third experiment, they took oxalate of potaſh with ſulphur. After the mixture was moiſtened and pulveriſed, it was treated with heat in a ſand-bath, and oxalate of lime was obtained.

As the above authors do not deſcribe this experiment, and give no proportions, I ſhall make known thoſe which I employed, and the reſult I obtained.

To get pure oxalate of potaſh, I ſaturated potaſh purified by alcohol with oxalic acid. I took 18 grammes of this ſalt, evaporated almoſt to dryneſs, and, having mixed it with 9 grammes of ſublimated ſulphur, moiſtened the mixture with half a decilitre of water. I then placed the capſule containing it on a ſand-bath, and evaporated it at a gentle heat. The reſiduum taken up by diſtilled water being filtered, there remained on the filter a blackiſh yellow reſiduum, and the liquor had aſſumed a ſlight yellow colour. The reſiduum was treated with nitric acid; but notwithſtanding the care employed in this operation, no oxalate of lime was found. The acid liquor abſolutely contained none of it. The ſulphur which remained was burnt in a platina crucible, and left no reſiduum whatever.

Hoping that this experiment would ſucceed better with a ſtronger heat, the ſame quantities of oxalate of potaſh and ſulphur were employed, and the mixture was placed in a platina crucible over a naked fire. Half an hour after, being brought to a red heat, it was taken off, and the maſs obtained was treated with diſtilled water, which aſſumed a dark yellow colour, and the real ſmell of an hydrogenated ſulphuret. The liquor was filtered, but no lime was found in the reſiduum. This reſiduum, which weighed only two centigrammes, was found to be charcoal, furniſhed in all probability by the potaſh employed, which had been purified with alcohol. I aſſured myſelf that all the oxalate of potaſh had been decompoſed, and that there had reſulted from it a ſulphate of potaſh.

Exp. IV. They put oxalate of ammonia into very pure oxygenated muriate of potaſh, and cauſed it to evaporate at ſeveral times, taking care to maintain an exceſs of nitric acid to diſſolve the oxalate of lime which was formed. In this manner a very large quantity was obtained.

This fourth experiment, which appears to be that which has hitherto given to the above authors the moſt lime, by the decompoſition of the hyper-oxygenated muriate of potaſh, was made with great care, but always without ſucceſs. As theſe authors do not ſpeak of the quantities employed, I ſhall give an account of my experiment.

I took 16 grammes of hyper-oxygenated muriate of potaſh, pure and cryſtallized, and 8 grammes of oxalate of ammonia, alſo pure and cryſtallized, and put them into a porcelain capſule, with two decilitres and a half of diſtilled water. The capſule being placed on a ſand-bath, the mixture was evaporated to dryneſs; and towards the end of the evaporation care was taken to add a gramme of nitric acid to diſſolve the oxalate of lime which might be produced. The evaporation was repeated four times ſucceſſively, adding water at each operation, and a gramme of nitric acid. The reſiduum was then taken up by a new quantity of diſtilled water, which completely diſſolved the whole of the matter. As there were ſome atoms diſſeminated throughout the liquor, it was filtered, but it was not poſſible to appreciate the reſiduum, as the liquor was very clear. The nitric acid was ſaturated with ammonia, to precipitate the oxalate of lime; but in vain was an exceſs added, for the liquor ſtill remained clear.

The ſame experiment performed cold did not exhibit more ſucceſsful reſults, though the mixture remained fifteen days in contact, as recommended by the authors above mentioned.

Exp V. The fifth experiment, which ſeems to have confirmed C. Guyton and Deſormes in the opinion they had formed of the converſion of potaſh into lime, is that which they performed with hyper-oxygenated muriate of potaſh and the phoſphoric acid. fter treating a mixture of 20 grammes of this ſalt with 10 grammes of acid till reduced to glaſs, they diſſolved it in water; and, having ſaturated the exceſs of acid by ammonia, filtered the liquor, expoſed it to evaporation, and made it again paſs to the ſtate of glaſs. By ſix different evaporations, they obtained two or three grammes of phoſphate of lime, as they believed.

1ſt, The hyper-oxygenated muriate of potaſh, decompoſed by the phoſphoric acid, obſerving the proportions above announced, produces, by evaporation to dryneſs, a vitriform matter, a part of which remains inſoluble in the water under the form of a granulated white powder brilliant like ſalt.

2d, The liquor ſeparated from the inſoluble part being again calcined, ſtill leaves a certain quantity of inſoluble powder ſimilar to the former; a third calcination produces the ſame effect; and ſo in ſucceſſion, till the matter is almoſt entirely converted into this inſoluble powder.

3d, If the calcination is performed in a ſtrong heat, and continued a long time, the quantity of the inſoluble powder is more conſiderable, and fewer calcinations are required to convert the matter into this powder.

It is ſeen by the three preceding paragraphs, that we obtained a great deal of this ſalt; it is indeed not difficult to procure it. We ſhall now deſcribe the experiments to which we ſubjected it, and which evidently proves that this ſalt is not phoſphate of lime.

4th, This inſoluble powder has no ſavour; it is inſoluble in cold water, but it is ſoluble in boiling water in notable proportions.

5th, Though this matter be little ſoluble in water, lime-water however produces in the liquor a very ſenſible precipitate. Nitrate of ſilver furniſhes one alſo, which is ſoluble in nitric acid.

6th, Ammonia occaſions no change in the ſolution of the matter in queſtion. As little effect is produced by the fixed alkalies.

7th, The oxalate of ammonia made no alteration in the above liquor, which it would certainly have done, had there been a ſuitable quantity of lime.

8th, This ſalt is exceedingly fuſible. By the blow-pipe it fuſes into a pearl, tranſparent while in a ſtate of fuſion, but which on cooling becomes opake.

9th, This powder is ſoluble in the nitric, muriatic, and even phoſphoric acids. Theſe ſolutions are thick, viſcous, and tenacious, like clear ſtarch. When theſe ſolutions are ſufficiently diluted with water, they give no precipitates by alkalies; but, if concentrated, they give one, which rediſſolves in a large quantity of water.

From all theſe experiments we are authorized to conclude the citizens Guyton and Deſormes took an alkaline phoſphate for phoſphate of lime. This phoſphate, which is ſcarcely ſoluble, as above ſeen, is not a ſimple phoſphate; for it is known that the alkaline phoſphates are very ſoluble, eſpecially that of potaſh; but it was not before known that in the experiment of which we have given an account there is formed a new phoſphate, which ought to be diſtinguiſhed by the name of neutral phoſphate. The following experiment ſerves alſo to confirm this reaſoning, and confirms in an irrevocable manner all thoſe deſcribed.

I mixed a certain quantity of phoſphate of potaſh with pure potaſh, placed the mixture in a platina crucible, and expoſed it to a ſtrong heat. I obtained for reſult a white ſubſtance, almoſt inſoluble, which poſſeſſed all the properties of the ſalt obtained in the laſt experiment, and which was again aſcertained to be a real phoſphate of potaſh with an exceſs of baſe. This kind of ſyntheſis ſtill proves that the phoſphate of lime deſcribed by the above authors is only this new ſalt, which, on account of its apparent properties and inſolubility, has been too little examined.

Exp. VI. By treating hyper-oxygenated muriate of ſoda in the ſame manner as that of potaſh, theſe authors announce that they extracted from ſix grammes of this ſalt one gramme of magneſia.

To repeat this experiment, the hyper-oxygenated muriate of ſoda was made by ſaturating with oxygenated muriatic gas a ſolution of pure carbonate of ſoda. When this ſalt was formed, it was evaporated to dryneſs in a porcelain capſule; and 6 grammes of it in this ſtate were put into a platina crucible, when fuſed phoſphoric acid was added in ſmall portions. The mixture immediately became yellow, and a great deal of oxygenated muriatic acid was diſengaged, as was the caſe in the experiments made with oxygenated muriate of potaſh. I continued to add phoſphoric acid till nothing more was diſengaged; upon which, a ſtrong heat being applied, the whole maſs became liquid. After cooling there was found a vitreous matter, brittle, ſlightly deliqueſcent, and wholly ſoluble in water. When the liquor had become very clear, an attempt was made to precipitate from it the magneſia, by the help of potaſh; but having obtained not a ſingle atom of precipitate, the experiment was repeated ſeveral times, but always without ſucceſs. From what has been ſaid, it is evident that the magneſia obtained by the above authors can not have ariſen but from the ſoda which they employed; for it is known that ſoda is very often accompanied with magneſia. In this experiment, as in the fifth, the platina is ſtrongly attacked. In two trials I had a reſiduum of eight centigrammes of this metal.

Exp. VII. Nitrate of ſoda treated by the phoſphoric acid gave to the above authors magneſia. I repeated this experiment four times without obtaining the ſame reſult. I varied, but in vain, the doſes of the ſalt and the acid, as well as the mode of operation; but by employing as much as 20 grammes at a time of nitrate of ſoda, and 10 of acid, I obtained only a blackiſh reſiduum weighing four centigrammes, which was found to be ſilex coloured by charcoal. Thoſe who recollect the exiſtence of this earth in the phoſphoric acid diſcovered by C. Vauquelin, will be able to account for the ſilex found in this experiment.

I muſt inform thoſe who may wiſh to repeat this experiment, that they muſt not employ phoſphorous acid for phoſphoric acid; for when the former is employed, and poured over the red and fuſed nitrate, a very ſtrong deflagration is produced, the matter is thrown from the crucible, and ſpeedily burns every thing that it meets with. This obſervation ought to be recollected in regard to the Experiments 5 and 6.

Exp. VIII. In this eighth experiment, when potaſh was heated with charcoal, and when the capſule containing the mixture was brought to a red heat, they obtained, for reſiduum, carbonate of potaſh, lime, magneſia, and even ſome alumine.

Without deſcribing all the fruitleſs trials made to find the three earths produced in this experiment, I ſhall relate ſome of them, which prove that theſe pretended converſions ariſe only from the impurity of the charcoal employed.

Having made choice of light charcoal from white wood, it was pulverized and ſtrongly calcined. Sixteen grammes of pure potaſh and eight grammes of this calcined charcoal were then exactly mixed, by moiſtening it with a little water; after which it was put into a platina capſule, and brought to a red heat. The reſiduum, after being edulcorated with water, to ſeparate from it all the potaſh, was treated with nitric acid. The ſolution, when filtered, gave by evaporation a glutinous matter, found to be ſilex. The nitrates ſeparated from this ſilex gave lime and magneſia.

Preſuming that the charcoal might be the cauſe of the preſence of the lime and magneſia found in this experiment, I thought it my duty to aſſure myſelf of it by analyſing that employed.

I burnt 216 parts of the ſame charcoal to reduce it to aſhes, taking care to employ a platina crucible; the aſhes obtained weighed only 91 parts. This reſiduum was treated with the nitric acid. By proper evaporations, the preſence of a great deal of carbonate and ſulphate of lime was aſcertained. The products obtained ſeparately by the common proceſſes gave ſilex 41, magneſia 16, lime 12, alumine 3.

For the ſake of greater preciſion, and to be more fully convinced in regard to the exiſtence of lime in the charcoal, as well as to prove that the lime found by C. Deſormes aroſe only from the charcoal employed, I made the following experiment:

I took four grammes of pure charcoal obtained from the decompoſition of the carbonic acid by phoſphorus, and, having mixed it with 18 grammes of pure potaſh, put it into a platina capſule, and brought it to a red heat, having previouſly taken care to moiſten it. The matter which thence reſulted being treated with water, there remained a black reſiduum, which was treated with nitric acid, and the ſolution diluted with water was filtered. This nitric ſolution was mixed with oxalate of ammonia; but this ſalt produced no alteration in the above liquor, even when the exceſs of acid was ſaturated by ammonia; which it could not have failed to do had there been ever ſo little lime, or even magneſia and alumine. This would be the place for calling the reader's attention to the difference of the phænomena by pure ſubſtances and thoſe which are not ſo; but I ſhall confine myſelf to mention that exhibited by the experiment in queſtion.

When the pure charcoal is treated with potaſh, as above ſaid; and when an acid is added to the ſolution ſeparated from the charcoal, in order to ſaturate the potaſh, no other phænomenon is obſerved than a diſengagement of carbonic acid gas, which the potaſh has abſorbed during the courſe of operation.

But, inſtead of pure charcoal, if that which we firſt employed be made uſe of, that is to ſay, charcoal of white wood; by ſaturating the potaſh employed, a very different reſult will be obtained. The acid, as ſoon as poured in, produces a diſengagement of carbonic acid. When a larger quantity is added, a great deal of ſulphurated hydrogen gas is produced; and by ſaturating the potaſh a large quantity of ſulphur is precipitated. This again explains the ſtate in which the lime is found, and perhaps the magneſia, in the charcoal; for there is no need to ſay that the ſulphur obtained in this operation muſt ariſe from the ſulphuric acid decompoſed. There is no doubt that the ſulphates of lime and magneſia contributed to this circumſtance, ſince the quantities of theſe earths in the charcoal were conſiderable. This ſtill ſerves to explain the obſervation made by C. Deſormes, who ſeveral times found lime, magneſia, and alumine in the ſame reſiduum. By recollecting that theſe three earths exiſt in charcoal, it will be readily ſeen whence they aroſe.

Exp. IX. By treating ſoda with charcoal in the ſame manner as the potaſh was treated, the above authors obtained magneſia, alumine, and lime. It is here ſeen that this reſult is the ſame as ought to be given by common charcoal. I ſhall therefore ſay nothing further reſpecting this experiment, than merely to announce that in treating pure ſoda with pure charcoal (that in particular ariſing from the decompoſition of the carbonic acid), none of the above earths are obtained.

Exp. X. Muriate of potaſh treated by charcoal gave to Deſormes lime. It is needleſs to obſerve, in this tenth experiment, that the lime aroſe from the charcoal; ſince, when pure matters are employed, none of that ſubſtance is found.

Exp. XI. The eleventh experiment, which, beſides lime, gave magneſia and alumine, will not give any of theſe earths when pure charcoal is employed. This experiment was made with muriate of ſoda and charcoal.

Exp. XII. The twelfth experiment, which gave exactly the ſame reſults as the eleventh, was made by treating muriate of ſoda with ſulphur. As charcoal had not been here employed, I took great care to repeat this experiment ſeveral times; but always without ſucceſs. I cannot preſume to ſay what may have been the cauſe of the reſult obtained by the above authors: it can be aſcribed only to the matters they employed, ſince, with pure muriate of ſoda and ſublimated ſulphur, I could diſcover, in the two experiments I made, no traces either of lime, magneſia, or alumine.

Exp. XIII. As the thirteenth experiment is of no uſe in regard to the theory of Guyton and Deſormes, it is needleſs to give any account of it.

Exp. XIV. By mixing ſulphate of ſoda with lime, theſe authors thought they perceived a diſengagement of ammonia: I alſo thought I obſerved the ſame diſengagement in the vapours produced by the muriatic acid when brought into contact with the mixture. Lime alone, though extracted from marble, gave by the ſame acid, when moiſtened, white vapours. I am not convinced that this diſengagement, ſenſible only on the approach of the muriatic acid, aroſe from ammonia; for, though I employed a large maſs of matter, I never could diſtinguiſh that alkali by the ſmell. The vapours occaſioned by the muriatic acid were perhaps owing only to the heat produced at the time of the mixtures, and alſo to the great affinity which the acid employed has for water. How, indeed, can we explain the formation of this ſubſtance by the combination of lime with pure water? or how can we conceive, if it is not formed, that it can co-exiſt with lime, which, as is well known, poſſeſſes the property of expelling ammonia wherever it is found?

Exp. XV. In the fifteenth experiment, theſe authors thought alſo that they remarked a diſengagement of ammonia, by combining intimately together lime, ſulphur, and water.

Exp. XVI. The ſixteenth experiment makes mention of an analogous trial as ſimple as the preceding. In my opinion, the explanation of theſe experiments will be ſufficiently developed in the eighteenth experiment, ſince it is that which, according to theſe authors, gave a great deal of ammonia.

Exp. XVII. By this experiment the above authors, inſtead of decompoſing the lime, attempted to compoſe it. The proceſſes which they employed for this purpoſe were as follow:

They formed nitrate of alumine, which they decompoſed, at ſeveral times, by heat: by this ſimple means they obtained lime.

Nitrate of magneſia was employed with the ſame ſucceſs.

After having carefully prepared the two nitrates, which they made uſe of for this compoſition, I took twenty grammes of the nitrate of alumine, and expoſed it to a ſtrong heat in a platina crucible. After three different and ſtrong evaporations to dryneſs, I obtained an earth perfectly white, and inſoluble in water. When treated with nitric acid, it was entirely diſſolved. This nitric liquor, which was very clear, was mixed with oxalate of ammonia; but this re-agent produced no change. The addition of ammonia, ſo as to ſaturate the acid, was tried, but in vain; it was only by an exceſs that a precipitate was obtained, but which, being ſoluble in potaſh, was found to be alumine; conſequently I did not find an atom of lime. It is true, that to prepare my nitrate of alumine I employed an earth perfectly pure, extracted from a natural ſtone at the time of my experiments on the affinity of earths. The acid employed was alſo very pure.

The nitrate of magneſia, notwithſtanding all my care, did not give a more ſatisfactory reſult: the lime found by Desormes was, no doubt, accidental.

Exp. XVIII. This experiment, which calls to mind the 14th, 15th, and 16th, on account of the ſame phænomenon by which they are connected, is that which increaſed the opinion of the above authors on the recompoſition of ſimple earths. Having inſtantaneouſly made ſulphuret of potaſh, and added oxalic acid, they obtained, as they announce, a ſenſible diſengagement of ammonia inſtead of ſulphurated hydrogen, and the reſiduum was magneſia.

After making two trials in a porcelain mortar without diſcovering ammonia, but a product extremely fetid, which I ſuſpected might conceal the alkali I ſought for, to aſſure myſelf of it I made the following experiments:

1ſt, I mixed 30 grammes of pure potaſh with 15 grammes of ſublimated ſulphur, and the mixture exactly made was put into a tubulated retort with two decilitres of water. To the beak of the retort I adapted a bent tube, which was immerſed in a ſolution of ſulphate of iron at its maximum of oxidation. By the diſpoſition of my experiment it is ſeen that I muſt have perceived the ſmalleſt portion of ammonia; for it is known, that if the ſulphurated hydrogen does not precipitate the ſulphate of iron green, an atom of this gas combined with ammonia will immediately manifeſt itſelf. My apparatus being thus arranged, I poured into the retort ſix grammes of pure and cryſtallized oxalic acid. A ſtrong diſengagement of ſulphurated hydrogen was immediately produced; the flaſk containing the ſulphate of iron was agitated a long time by this gaſeous fluid; but when the operation was finiſhed there was not a ſingle atom of precipitate produced in the liquor. This experiment, which I think one of the moſt exact for aſcertaining the ammonia that might have exiſted or been produced in this operation, evidently proves, that if the above authors obtained any, this alkali muſt have ariſen from ſome other cauſe than that of a decompoſition of the ſubſtances employed, if they were pure.

The reſiduum examined in three operations gave no magneſia. I muſt here obſerve that I employed ſulphur, which I ſublimated myſelf.

To remove all doubt reſpecting this experiment, and to be more certain of my reſults, I made a ſecond, but ſtill without ſucceſs.

The apparatus being arranged as in the preceding experiment, I put in the place of the ſulphate of iron muriatic acid, and began my operation. After all diſengagement had ceaſed, I took the muriatic acid, which I evaporated at a gentle heat; but, not being able to obtain an atom of ſaline reſiduum, I poured lime into the ſmall quantity of liquor which remained. I added more than was neceſſary, but notwithſtanding all my precautions I had no diſengagement of ammonia. It is now ſeen, by theſe two experiments, that no diſengagement of ammonia is produced; for, I muſt repeat it, muriate of ammonia would have been formed in the laſt, and in the firſt iron would have been precipitated: nothing is diſengaged but ſulphurated hydrogen gas in very large quantity, and the reſiduum contains no magneſia.

Exp. XIX. This laſt experiment the above authors made by taking the charcoal of ſugar, which they mixed with lime extracted from marble: this mixture being expoſed to a ſtrong heat in a platina crucible well ſtopped, they obtained alumine and magneſia.

As they do not give the proportions of the matters employed in this experiment, nor the quantities of the products obtained, I preſume that they conſidered it only as a new proof in favour of the theory they have eſtabliſhed.

I repeated this experiment, and employed charcoal of ſugar and pure charcoal. With the charcoal produced from 40 grammes of cryſtallized ſugar, I obtained a little alumine: the product collected weighed only ſix centigrammes; perhaps it contained magneſia. In my ſecond operation, when I employed pure charcoal, I had not a ſingle atom of the above-mentioned earths: it is therefore ſeen that the products obtained in this experiment aroſe from the impurity of the matters employed.

Concluſion.

If the experiments I have deſcribed in the courſe of this memoir are exact, and if I have properly obſerved the phænomena they exhibited, the reſult will be, that the facts announced by C. Guyton and Deſormes, though for the moſt part true, cannot lead to the concluſions which they have deduced from them, on account of the nature of the ſubſtances which they employed: we muſt therefore conclude, till otherwiſe ſhown, that the alkalies and earths muſt be retained in the claſs of ſimple or indecompoſed bodies; that the experiments of theſe chemiſts do not prove that potaſh is formed of lime and hydrogen; that magneſia and azote produce lime; that azoted alumine conſtitutes magneſia; and, in the laſt place, that magneſia and hydrogen give birth to ſoda. I do not, however, pretend that theſe ſubſtances are ſimple, but only that the facts adduced as a proof are not proper to prove it.

LVII. *Extract of a Letter from* M. VON HUMBOLDT *to* LALANDE.

Caraccas, Dec. 14, 1799.

A FEW weeks after my arrival in South America, I tranſmitted to Delambre an extract from my aſtronomical obſervations, becauſe I hoped that ſome of them might be intereſting to the Board of Longitude: as I have, however, learned that the brig by which I ſent my letters was wrecked near Guadaloupe during the ſtorm which lately prevailed in theſe regions, I think it neceſſary to tranſmit to you a copy of them.

After a paſſage of about ſix weeks, in the frigate Pizarro, I arrived at the coaſt of Paria. My plan was to proceed firſt to the Havannah, and thence to Mexico; but I could not withſtand the ſtrong deſire I had of ſeeing the wonders of the Oronoko, and the lofty Cordilleras, which extend from the high land of Quito to the rivers of Guarapeche and Arco. As I have now returned from a very intereſting journey to the interior of Paria, through the Cordilleras of Cocolar, Tumeri, and Guiri, and to the ſettlement there of the Capuchins, never before viſited by any naturaliſt, during which I carried with me, on three mules, my aſtronomical inſtruments, viz. a quadrant by Bird, and ſextants, teleſcopes, and micrometers, by Ramſden and Troughton, you will perhaps expect that I have done a great deal for aſtronomy; but you know that this ſcience is too remote from my principal objects— philoſophy in general, geology, eudiometry, and the phyſiology of plants and animals; and under the latitude of 10° it is impoſſible to labour ſo inceſſantly as under 49°. I choſe rather to make a few obſervations with accuracy, than a great many imperfect ones; and to inſert them with great minuteneſs in my journals, that, in caſe I ſhould die in the courſe of my travels, no doubt might remain in regard to their exactneſs.

In the two papers which I tranſmitted to Delambre, you will find my obſervations made in Europe with Borda's new *inclinatorium.* I found by it, that local circumſtances have a greater influence on the inclination of the magnetic needle than on its declination; and that no connection ſeems to exiſt between the ſituation of the place of obſervation and the inclination. This I find confirmed in the new world in the interior of New Andaluſia, and therefore the obſervations made by Nouet in Egypt ſeem to be eſtabliſhed. The declination, however, is affected by local cauſes, but much leſs: at ſea they are much more regular, and the variations are much more uniform.

I have here given you my obſervations of the inclination only, which are true to 15′. During calms I obtained them at ſea with more accuracy, and the periods of the vibrations of the needle could be determined very exactly. If the ſame number is counted in the ſame time during five or ſix repeated trials, and if this is ſtill the caſe when the inſtrument is moved from its place, the reſults, in my opinion, may be conſidered as correct. Though calms are not uncommon between the tropics, during the courſe of forty days I was able to make only ten very accurate obſervations.

Places of Obſervation, 1799.	Latitude.	Longitude from the First Meridian.	Inclination.		Magnetic Power.
			Centigrade Degrees.	Old Degrees.	Number of Vibrations in Ten Minutes.
Paris - - - -	48° 50′ 15″	20° 0′ E	77·15°	69° 28′	245
Nîmes - - - -	43 50 12	21 59	72·65	65 23	240
Montpellier - -	43 36 29	21 32·5	73·20	65 53	245
Marſeilles - - -	42 17 49	23 3·5	72·40	63 10	240
Perpignan - - -	42 41 53	20 33·5	72·55	65 18	248
Barcelona - - -	41 23 8	19 52	71·80	64 37	245
Madrid - - -	40 25 18	13 58	75·20	67 41	240
Valencia - - -	39 28 55	17 29	70·70	63 38	235
Medina del Campo	——	——	73·50	66 9	240
Guadarama - -	——	——	73·50	66 9	240
Ferrol - - - -	43 29 0	9 24·5	76·15	68 32	237
In the Atlantic Ocean between Africa and America.	38 52 15	3 40	75·18	67 40	242
	37 14 10	3 30	74·90	67 30	242
	32 15 54	2 52·5	71·50	64 21	——
	25 15 0	0 36 W	67	60 18	239
	21 36 0	5 39	64·20	57 49	237
	20 8 0	8 34	63	56 42	236
	14 20 0	28 3	58·80	52 55	239
	12 34 0	33 14	50·15	45 8	234
	10 46 0	41 24	46·40	41 46	229
	10 59 30	44 31·5	45·50	41 57	237

In the year 1776, according to Cavallo, the inclination of the needle in the undermentioned latitudes and longitudes was as follows:

Latitude.	Longitude.	Inclination.
24° 24′	18° 11′	59°
10	22 52	44 12
0	37 38	30 3

Since Coulomb and Caſſini no longer employ themſelves with obſerving the declination, I know no place in the earth where the declination has been determined with certainty to 10″, and not ten where it has been determined to 1′. What uncertainty ſtill prevails reſpecting the real declination of the needle at Paris! The ten obſervations which I made at ſea, will ſerve hereafter to determine whether the inclination ſpeedily changes. The longitude and latitude of the places of obſervation were always accurately determined at the time by means of a Ramſden's ſextant, divided from 15 to 15 ſeconds; and a Berthoud's chronometer. You will ſee by them that the inclination decreaſes very rapidly from the latitude of 37°, and that it increaſes leſs towards the eaſt than to the weſt from the latitude of 37° to 48°.

It appears to me, that in the higher chain of the calcareous mountains of this province, not far from the equator, ſmall elevations above the level of the ſea derange the inclination much more ſtrongly than the higher mountains in the Pyrenees and Old Caſtile. As a proof, I ſhall here give obſervations made at four places which lie pretty nearly in an arch from north to ſouth (comprehending 24′.)

Places.	Height above the Sea, Paris Feet.	Inclination.		Vibrations in Ten Minutes.
		Centigrade Degrees.	Old Degrees.	
Cumana	24	44·20°	39° 47′	229
Zueteppe	1111·2	43·30	38 58	229
Impoſſibile	1470·	43·15	38 50	233
Cumanacoa	636	43·20	38 53	228
Cocollar	2352	42·60	38 20	229

Borda is of opinion, as appears by the memorandum drawn up for the men of ſcience who accompanied La Perouſe, that the intenſity of the magnetic power is the ſame all over the earth; as he aſcribes the ſmall variation which he found in it at Cadiz, Teneriff, and Breſt, to the imperfection of the compaſs. He requeſted me to verify this circumſtance. You here ſee that the magnetic power is ſo different, that between Paris and Cumana it decreaſed from 245 to 229 vibrations in ten minutes, though it does not decreaſe with the inclination. This decreaſe cannot certainly be aſcribed to any change in the goodneſs of the needle, or to other accidental cauſes: for the ſame needle made in the ſame time at Paris 245 vibrations; at Girona, 232; at Barcelona, 245; at Valentia, 235; and, after a journey of ſeveral months, gave at the ſame places exactly the ſame number of vibrations as before my departure. Theſe are always the ſame, in the open fields, in the houſe, or in a cavern, ſo that the magnetic power in any place is always the ſame, and remains a long time without change; and ſeems to be a general power, like that of gravity.

I had the mortification of not being able at ſea to make any good obſervations of the declination. Notwithſtanding all the trouble I took, I could not find an azimuth compaſs on which I could depend within 40′. This is the true reaſon why I have made no mention to you of the declination at ſea. The point, however, where the inclination vaniſhes, certainly lies further to the weſt than marked in Lambert's chart, in the Berlin aſtronomical almanac for 1779. A good obſervation, made in the year 1775 aboard an Engliſh ſhip from Liverpool, places this vaniſhing point in latitude 29° north, long. 66° 40′ weſt. I obſerved, with great care, the declination at two places on the coaſt of America with a compaſs by Lenoir, in which the needle is ſuſpended by a thread, according to the method of Prony and Von Zach. At noon, October 1799, it was at Cumana, and twenty leagues further eaſt, 4° 13′ 45″ eaſt: at Caripe, the chief place of the Capuchin miſſion among the Chaimas and Caribs, 3° 15′ eaſt.

During the earthquake at Cumana, on the 4th of November 1799, the inclination of the magnetic needle was altered, but not the declination. The former, before the ſhock, was 44°·20 of the new diviſion; after the ſhock it remained at 43°·35. The number of the vibrations of the dipping needle, however, was the ſame as before, viz. 229 in ten minutes. This, united to other obſervations, ſeems in my opinion to prove, that during the earthquake this ſmall part of the ball of the earth was changed, and not the needle; for, in diſtricts where no ſigns of an earthquake ever appeared in the primitive chain of accumulated maſſes of granite, the inclination was as great after as before.

On account of the intereſt which you take in every thing that relates to navigation, the following obſervations will not be unacceptable.

I have carefully proved what Dr. Franklin and Captain Jonathan Williams aſſert in the Tranſactions of the American Society * reſpecting the uſe of the thermometer for diſcovering ſhallows at ſea; and am able to confirm, in the fulleſt manner, what they have ſaid. I was aſtoniſhed to ſee how the water became evidently colder as its depth decreaſed, and how the neighbourhood of ſhallows, and of the coaſts, could thereby be announced. The worſt ſpirit of wine thermometer, if only ſenſible, will therefore, in the hands of the moſt ignorant mariner, be a very uſeful inſtrument in the night-time, and during ſtorms, or when it is difficult or impoſſible to heave the lead. This obſervation I cannot too ſtrongly recommend to the attention of the Board of Longitude. Our whole crew were aſtoniſhed to ſee how ſpeedily the thermometer fell, when we approached the large bank which extends from Tobago to Grenada, and eaſt from Margarita. Theſe obſervations may be made with more eaſe, as the water of the ſea, in an extent of 12000 ſquare miles, has always the ſame temperature day and night; ſo that the moſt ſenſible thermometer, during from four to ſix days ſail, does not riſe or fall above 0·3 . In the neighbourhood of ſhallows it was from 2 to 3 degrees, and more, colder. This obſervation of Franklin, hitherto forgotten, may at ſome period be of great uſe to navigation; not that ſeamen ſhould throw aſide the lead entirely, and truſt to the thermometer, for this would be folly; but becauſe the obſervations may be ſo eaſily repeated, and becauſe the thermometer will announce the danger much ſooner than the lead, as the colder water above the ſhallows leſſens the temperature in the water in the neighbourhood. I can aſſert that this new mean is not more uncertain than the log, and the helps already employed in navigation. If the thermometer does not fall, navigators ought not to depend that they are entirely ſecure from ſhallows; but if it falls, they muſt be on their guard. This warning is certainly more valuable than our ſea charts, where the ſhallows are in general laid down in a very incorrect manner; and to immerſe a thermometer in a bucket filled with ſea water is certainly much eaſier. I have alſo meaſured ſeveral times the ſpecific gravity and temperature of the ſea water, at the ſurface and at certain depths, by means of Dollond's balance and thermometers, which are placed in caſes furniſhed with a valve. As my inſtruments were compared with the beſt Pariſian ones, and as I could be more

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* Vol. iii. p. 32.

certain in regard to my longitudes than is uſual, the ſmall chart in which I intend to make known the reſult of theſe experiments will be intereſting. In the latitude of 17° and 18° north, there is a zone in the ſea between Africa and the Weſt Indies, where, without an extraordinary current prevailing, the water is denſer than under a leſs latitude. The following are ſome of my reſults reſpecting the temperature of the ſea water:

North Lat.	Long. from the Firſt Meridian.	Temperature	
		of the Sea Water at the Surface.	of the Air.
43° 29′	9° 29′ E.	12°	18°
39 10	3 41·5	12	13
36 3	2 57	12	14
35 8	2 15	13	16.5
32 15	2 52·5	14·2	13·5
30 35	3 6	15	16
28 55	2 37·5	15	17
26 51	— 47	16	15
20 8	8 33 W.	17	16
18 53	10 5	17·4	17
18 8	13 2	17·9	19
17 26	15 26	18	16
15 22	22 49	18·5	20
14 57	24 40	19	17
13 51	30 2·5	19·8	18·9
10 46	41 24	20·7	20·3
10 28	46 31	21	17 to 27
10 29	46 35	17·8	23

This letter was written at Cumana, but for want of a proper opportunity I was obliged to bring it with me to this large capital of the province of Caraccas, which is ſituated in a fertile valley abounding with cacao, cotton, and coffee, 2400 feet above the level of the ſea, and in a climate perfectly ſimilar to that of Europe. The thermometer in the night-time falls to 11°, and in the day never riſes above 17° or 18°.

The cloudy weather, which here daily follows the ſun's paſſage over the meridian, makes the obſervation of correſponding altitudes very difficult and uncertain, as the afternoon obſervation is often loſt. The cloudy weather after the earthquake of Cumana deprived me of an opportunity of obſerving the immerſions of the ſecond ſatellite of Jupiter on the 2d and 9th of November. I ſhall here give you the principal determinations of the longitude, which I made by means of Berthoud's chronometer, from the obſerved horary angles.

	Long. weſt.		
Cumana, caſtle of St. Antonio, lat. 10° 27′ 37″ and (taking the long. of Madrid at 13° 58′)	46°	31′	
Puerto Eſpana, in the iſland of Trinidad	43	49	30″
Tobago, the eaſtern extremity -	42	47	30
Macanao, the weſt part of the iſland St. Margarita	46	35	30
Punto Araya, in New Andaluſia -	46	35	30
Iſland of Coche, eaſtern extremity -	46	12	
Bocca de Drago (not certain) -	44	23	
Cabo de tres Puntas - -	44	54	30

Caraccas à la Trinité 10° 31′ 4″ latitude, very good.

I ſhall here alſo mention, that neither wind, ſtorms, nor the earthquake, have had the leaſt influence on the regular daily variation in the ſtate of the barometer; and that, according to C. Richard, this daily variation amounts at Surinam to two lines.

Theſe obſervations are the more intereſting, as all the charts of this part of the world are very incorrect; and the agreement of my longitude of Teneriff and Tobago, with the obſervations of Borda and Chabert, within from 2″ to 5″ of time, is a proof of the excellence of my chronometers.

The ſerenity of the nights between the tropics gave me an opportunity of comparing with each other the ſtrength of the light of the auſtral ſtars; which in ſome of them, ſuch as the Crane, Altar, Toucan, and the Foot of the Centaur, ſeems to have changed ſince the time of La Caille. I employed the method propoſed by Dr. Herſchel, and diaphragms of the ſame kind as thoſe uſed for the ſatellites. If the light of Sirius be ſuppoſed equal to 100 parts, and that of Procyon 88, the light of the following ſtars, according to my obſervations, are as below expreſſed.

	Parts.		Parts.
Canopus -	98	α in the Phœnix -	65
α in the Centaur -	96	α in the Peacock -	78
Acharnar - -	94	α in the Crane -	81
α in the Indian -	50	β - - -	75
β - - -	47	γ - - -	58
α in the Toucan -	70		

LVIII. *De-*

LVIII. *Deſcription of a portable Barometer. By* Dr. J. Guerin, *of Avignon, Profeſſor of Natural Hiſtory in the Central School of Vaucluſe, and Member of ſeveral Societies* *.

No philoſophical inſtruments are ſo varied in their conſtruction as barometers, and eſpecially the portable ones. But if the latter leave no room for improvement in regard to exactneſs, they are exceedingly complex, and liable to be deranged; ſo that travellers are often obliged to renounce the obſervations they depended on making during the courſe of their journeys.

It is very rare that the beſt barometers are not either deranged or broken in the courſe of a long journey. Nothing however but a violent fall can break the one which I am about to deſcribe. It may be tranſported on any carriage, and along the worſt roads, provided care be taken to keep it inverted; that is, with the cup uppermoſt. Naturaliſts who travel ſtood in need of this ſimple inſtrument, the only barometer really portable being that of Deluc, the conſtruction of which is more difficult; and which, in regard to obſervation, is liable to inconveniences which in mine I have endeavoured to avoid.

I ſhall ſay nothing of a great number of other barometers more or leſs complex, as well as defective, and in general inconvenient for tranſportation. They all have faults, which it would be too tedious to enumerate. It has been propoſed to uſe for this purpoſe iron tubes with glaſs tubes cemented to them; but it has not been remarked that the mercury at length oxidates, loſes its brilliancy, and adheres partially to the tube.

My barometer, the ſimpleſt poſſible, will be attended with this advantage, that it can be procured at a moderate price. The deſcription of it is as follows: A, B, C, (Plate IX, fig. 2.) is a glaſs tube about 33 inches in length, bent from B to C in ſuch a manner as to leave the diſtance of an inch between its branches.

D is a ſtop-cock, made of iron, ivory, box-wood, or any other ſubſtance not ſuſceptible of being attacked by mercury, and which ſerves for intercepting at pleaſure its paſſage into the cup. Box-wood, dipped in boiling wax, appears to me very proper for making a good ſtop-cock, as it can permit the mercury to paſs between the cock and its key-ſtopper (if the mercury contained in the tube ſhould dilate too much), without ſuffering it to eſcape when it does not dilate. Ivory ſtop-cocks are attended with the ſame advantage. E is a cylindric cup, the diameter of which muſt be known; about an inch appears to me to be the moſt convenient dimenſion for a barometer of this kind.

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* From the *Journal de Phyſique*, Frimaire an. 10.

F is a piece of ſhamoy leather with which the cup is covered. It permits the air to exerciſe an action on the mercury, and prevents it from eſcaping when the inſtrument is carried.

After having boiled the mercury in the tube, the cock and cup muſt be cemented at the interior bending. A little mercury is then to be added, that it may float above the ſtop-cock, when it is ſhut after the inſtrument has been inclined.

It may readily be conceived that the interior diameter of the long tube, and that of the cup, being known, the greateſt preciſion may be obtained when they are perfectly cylindric; for, the diameter of the two cylinders being known, the ratio of their ſolidity may be known *.

Thoſe who wiſh to avoid the trouble of this ſhort calculation may cement to the ſtop-cock a very large cup; and then the error occaſioned by the elevation of the level of the mercury will be a very ſmall fraction of a line, even if the barometer ſhould deſcend ſeveral inches.

In caſe it may not be thought proper to adapt to the ſtop-cock a large cup, which would render the barometer much heavier, nothing will be neceſſary but to meaſure the height of the mercury above the line of the level, and to ſubtract it from that of the mercury in the long tube. Let us ſuppoſe, for example, that the barometer marks 23 in. 6 lin. and that the mercury in the tube is elevated only one line; the remainder will be 23 in. 5 lin. for the height of the mercury.

By taking a view of the height of the barometer, without making the leaſt calculation, it will be eaſy to deviſe how the ſcale may be graduated ſo as to make this correction needleſs.

A nonius, to indicate the $\frac{1}{20}$th or even $\frac{1}{100}$th of a line may be adapted to this inſtrument; but the variations in the upper ſtrata of the atmoſphere are too well known to admit of our having recourſe to ſo exact a graduation in the meaſuring of heights. Ought we not to conſider as very correct a barometer that indicates $\frac{1}{10}$th of a line? The errors in the meaſure-

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* Let us ſuppoſe that the diameter of the barometric tube is two lines, and that of the cup twelve, the aſcenſion of the mercury in the cup will be to the depreſſion of that in the tube as the ſquare of the ſurfaces; which in this caſe is as 4 to 144, or 1 to 36. When the barometer indicates 25 inches, we muſt then ſubtract from that height 1-36th, that is to ſay, a line, and we ſhall have the real height, 24 inches 11 lines.

ment of great heights ariſe rather from the unequal denſities of the ſtrata of the atmoſphere, and the attraction of the mercury in the tube, than from defects in the ſubdiviſion. A barometer will always be pretty good, if the mercury it contains is well purified and dry; and if it has been boiled in the tube; if the tube is at leaſt $\frac{3}{4}$ths of a line in diameter; if, by a proper conſtruction, the air is prevented from introducing itſelf into it, notwithſtanding the agitation it is expoſed to in the courſe of a long journey; and, in particular, if the interval between the ſurface of the mercury contained in the tube and that in the cup can be exactly meaſured.

This barometer differs very little from that of Deluc; but it is much ſimpler in the conſtruction, and leſs expenſive. The cup which I have ſubſtituted for the tube, covered with leather, ſerves to retain a little mercury in the reſervoir, that no vacuum may remain between the ſtop-cock and the bending, when the barometer is inclined for the purpoſe of being tranſported. As it is not deep, and has a cylindric form, it may be eaſily cleaned in caſe the metal ſhould become oxidated. Deluc acknowledges that his barometer is inconvenient for daily obſervation. The one here deſcribed has not the ſame fault. It is both portable and fit for the cloſet. I have already ſaid that, neglecting all correction, the error reſulting from the difference of the levels would amount only to $\frac{1}{32}$d—a difference which may be neglected in making meteorological obſervations.

One of the greateſt advantages of this inſtrument is, that it may be rendered comparative, by chooſing tubes of about two lines diameter, and cylinders of an inch. It is of no importance whether a cup-barometer maintains itſelf a little higher or a little lower than a ſyphon one, if the depreſſion is conſtant, and its progreſs regular. I have carried both barometers on mountains more than 1800 toiſes in height.

I made a great many experiments on this ſubject on Mount Ventoux. Mine I confeſs always maintained itſelf a line lower; but was this depreſſion of a line a fault of the cup-barometer, or did the fault conſiſt in the greater elevation of the other? I do not know whether the ſolution of this problem would be favourable to Deluc's barometer, or to mine.

If the barometric obſervations do not require too much time and preparation; and if a ſimple, ſtrong, and cheap conſtruction ſupply the place of complex inſtruments liable to be deranged; I can aſſert that all theſe advantages are united in this barometer, by means of which correct obſervations may be made, which will ſupply our ſcientific inſtitutions with portable and comparative inſtruments, and with which the level of the whole ſurface of France might be taken without any ſenſible error, and without having recourſe to long and tedious operations. The ſociety of Manheim ſent meteorological inſtruments to various diſtant obſervers; but the conſtruction of them rendered this excellent plan fruitleſs; as the barometers, when they arrived, were all broken or deranged.

Would it not be of advantage to ſcience, if the French government would ſend to all the ſchools, learned ſocieties, and other eſtabliſhments, barometers, by means of which comparative meteorological tables might be conſtructed; by which the height of the mountains and different poſitions might be aſcertained, and valuable obſervations made, which have been omitted in moſt topographical ſurveys, or have been made ſo badly, in conſequence of the inaccuracy of the barometers, that no dependance can be placed on them?

The advancement of philoſophy and natural hiſtory is the object I propoſed in publiſhing theſe reflections. Comparative obſervations might render the ſtudy of meteorology leſs dry and more agreeable. The want of correct obſervations is the cauſe of our being as yet ſo little acquainted with the extent of the variations in the atmoſphere, the point of their maximum or minimum, &c. &c. There is reaſon to think that ſuch data would increaſe the probability of meteorological prognoſtication, and render more intereſting a branch of natural philoſophy yet little known. The correſponding heights of mountains and plains might be of great ſervice to the geologiſt, and enable him to reſolve the moſt curious problems.

P. S. I have learnt, from C. Dolomieu, that profeſſor Pictet employed a barometer nearly ſimilar to that here deſcribed. But as I am unacquainted with that inſtrument, and have uſed mine in journeys for four years without its being deranged, I publiſh this deſcription of it with confidence, being perſuaded that it may be of uſe to philoſophers and naturaliſts.

LIX. *Ana-*

LIX. *Anatomical Obſervations on the Structure of the Ornithorynchus Paradoxus.* *By* J. F. BLUMENBACH, *Profeſſor of Medicine at Göttingen, and Member of the Medical Society of Paris*[*].

THE ſpecimen of this animal in my poſſeſſion is about 19 inches in length; the head is 4 inches; the tail nearly the ſame; and the neck and trunk 10 inches[†].

It is covered with two kinds of hair. The interior gray, ſhort, and very ſoft, like down; that on the outſide, mixed with others longer and ſtiffer, browniſh on the back, and yelloviſh on the belly.

The limbs are ſhort. The fore feet are about $2\frac{1}{2}$ inches in length; the hind feet are ſomewhat longer. The feet have five toes, palmated (*natatorii*). The natatory membranes of the fore feet are very broad, not inſerted between the toes, but attached below them, ſo as to be better calculated for diving.

The tail flat, and covered with ſtiff hair like briſtles.

But what appears moſt ſingular and anomalous is the conformation of the head, being furniſhed with a broad beak reſembling that of a duck. The mandibles, ſpatula-formed, are flat. The upper one, about 2 inches in length, and $1\frac{1}{3}$d in breadth.

Theſe mandibles are covered with a coriaceous membrane, which extends to the corners of their aperture.

The under mandible, which is narrower than the upper, is ſerrated (*a*, fig. 1. Plate IX.) on the edges, as is the caſe in a duck's bill. The palate is furrowed acroſs.

My ſpecimen is deſtitute of teeth, like that deſcribed by Dr. Shaw; but I have lately been informed, in a letter from Sir Joſeph Banks, that in another ſpecimen two ſmall *dentes molares* have been found on each ſide of each mandible.

The whole beak at the root is edged with an undulated membranaceous border running acroſs it (*b*).

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[*] From *Mémoires de la Société Médicale d'Emulation, quatrième année.*

[†] Dr. Bobba, a very reſpectable phyſician now reſident at Göttingen, had before ſent us a very intereſting note on the *ornithorynchus paradoxus*, in which he ſays that there are three ſpecimens in Great Britain ſimilar to that in the poſſeſſion of profeſſor Blumenbach; viz. one in the Britiſh Muſæum; one in the poſſeſſion of Mr. Dobſon, an amateur of natural hiſtory at London; and a third in the univerſity of Edinburgh. But the information he ſent us having been already publiſhed in the *Bulletin des Sciences*, we did not think it neceſſary to inſert it in this work. We have confined ourſelves therefore to theſe new obſervations of profeſſor Blumenbach.—*Note of the French Editor.*

The conformation of the cranium, taken from the occipital condyli (*cd*), and the intermaxillary bones (*ef*), has, on the firſt view, a great reſemblance to that of the duck. The bones of the ſkull are divided by no ſutures; but the interior chamber of the brain is divided by an oſſeous hook (*g*) running lengthwiſe, which is found in no other quadruped [*], and ſimilar to that of the *tetrao urogallus*.

What ſeems to be highly worthy of notice, however, is the remarkable apparatus of the nerves of the ſecond branch of the fifth pair, and the diſtribution of it in three regions of the coriaceous membrane which ſurrounds the beak. The nerve (*h*) which proceeds from the lower orbital foramen goes to the tranſverſe edge: the other (*i*), which iſſues behind the intermaxillary bones, is diſtributed on the labial edges: the third (*k*), which iſſues from the ſynchondroſis, that divides the anterior hooked legs of the intermaxillary bones, proceeds to the integument of the beak.

Comparing this large apparatus of maxillary nerves, with which nature has provided this coriaceous integument, and the like ſtructure of the duck's bill, there can ſcarcely be a doubt that this highly ſenſible membrane, which ſurrounds the beak, ſerves, in a very particular manner, in the *ornithorynchus*, as in aquatic birds, for the purpoſes of feeling, by means of which theſe animals can ſearch for their food in muddy places, where neither ſight nor ſmell can be of any uſe to them in that reſpect. Hence the eyes of the *ornithorynchus* are very ſmall, but the noſtrils (*l m*) exceedingly broad.

This wonderful animal, therefore, affords an exception to the order of the internal ſenſes aſſigned to quadrupeds by Buffon [†], who ſays: "In quadrupeds, ſmell is the firſt of ſenſes; taſte is the ſecond; or rather, theſe two form only one: ſight is the third; hearing the fourth; and feeling the laſt."

In the annexed figure, one of the bones of the ſkull is repreſented broken, that the interior of the chamber of the brain may be ſeen. N, denotes the orbita; O, the zygoma; P Q, the proceſſus molares of each mandible. The other parts will be underſtood from the preceding deſcription.

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[*] I once found, but the inſtance is very rare, a ſimilar oſſeous hook in the cranium of a woman about thirty years of age.

[†] Hiſtoire des Oiſeaux, tom. i. p. 48.

LX. *On the Analyſis of Wine.* *By* C. CHAPTAL.

[Concluded from p. 142.]

NOW that we are acquainted with the faults in the conſtruction of the diſtilling apparatus, let us try to make an application of it in order to improve the knowledge we have acquired in regard to diſtillation, and the art of managing the fire.

It appears to me that the whole art of diſtilling may be reduced to the three following principles:

1ſt, To heat at once, and in an uniform manner, all the points of the maſs of the liquid.

2d, To remove all thoſe obſtacles which may confine the aſcent of the vapours.

3d, To effect the condenſation as ſpeedily as poſſible.

To fulfil the firſt condition, it will be neceſſary that the maſs of the liquid ſhould have little depth; and this requires that the bottom of the boiler ſhould preſent a very large ſurface, that the fire may be applied in a great many points.

The bottom of the ſtill ought to be a little bent inwards. This form is attended with two advantages: the firſt is, that by theſe means the fuel is at an equal diſtance from every point of it, and that the heat is uniform throughout; the ſecond is, that by this conſtruction the bottom of the boiler has more ſtrength, and the matters which may be depoſited at the bottom of the liquor are thrown to the angles which reſt on the maſon work, and where, conſequently, it is leſs dangerous. When this depoſit falls upon parts immediately expoſed to the direct action of the heat, it forms a cruſt, which prevents the liquor from being in contact with the part of the boiler covered by it, and the fire then burns the metal. This inconvenience is not to be feared, if the depoſit by the curved form of the bottom is thrown into the angles, where it is withdrawn from the direct action of the heat.

The fire muſt be made to circulate around the boiler by means of a ſpiral chimney; in which caſe none of the heat will be loſt, as the whole liquid is ſurrounded by it, and expoſed to its action in an uniform manner.

That the column of ſteam which riſes may experience no obſtacle in its aſcent, the ſides of the boiler ought to riſe in a perpendicular direction; and the ſteam muſt be maintained at the ſame degree of expanſion till it has reached the refrigeratory. But the ſteam riſing freely, and being condenſed by coming into contact with the cool ſides of the capital, would fall back into the boiler or body of the ſtill, if the ſides were not inclined in ſuch a manner as to make the drops of the liquid, which attach themſelves to them, trickle down into the gutter which conveys them into the worm. I have found by calculation that this inclination ought to be at leaſt 75 degrees to the horizon. It is alſo neceſſary that the water of the refrigeratory ſhould be frequently renewed; otherwiſe it will ſoon acquire the temperature of the ſteam, and be no longer able to condenſe it.

Though theſe principles in regard to diſtillation are inconteſtable, ſome modifications muſt be made in them for the purpoſe of facilitating the operation: by giving to the aperture of the boiler a diameter equal to that of the baſe, the capital becomes very wide; it is conſequently neceſſary to give it a conſiderable height, that its ſides may have the inclination of 75 degrees. This conſtruction is attended with two great inconveniences: the firſt is, that it renders the capital heavy, clumſy, and expenſive; the ſecond, that it makes it difficult to give to the upper edges of the boiler that ſtrength neceſſary to withſtand the effort of the capital. Theſe conſiderations induced me to make ſome changes in the above conſtruction. Theſe changes all relate to the form of the boiler: I make the ſides to project a little as they riſe, and then bring them nearer to each other towards the top, in ſuch a manner that the diameter of the aperture may correſpond to that of the bottom. This form removes the two effects above mentioned, and has the advantage of preſenting a border at the upper part, againſt which the bubbles ariſing from too ſtrong an ebullition may be broken, and thrown back to the centre of the boiler.

Beſides this change in the form of the boiler, I thought it proper to take away the refrigeratory with which the capital was covered, and which was attended with the inconvenience of cooling the ſteam, and forming a cloud of vapour in the inſide, which oppoſes its further aſcenſion.

It may be obſerved, that in diſtilling by means of a retort in a ſand-bath, it will be ſufficient to apply a cool body to the retort to produce this effect: ſtriæ are immediately ſeen formed on the ſides, and the liquor falls to the bottom of the retort.

If I at firſt propoſed to preſerve the refrigeratory, it was becauſe I aſcribed to it a part of the effects which depended on a proper conſtruction of the furnace. I, however, afterwards found that a ſtill greater effect was obtained by ſuppreſſing the refrigeratory. Beſides, this change was more economical, and occaſioned leſs embarraſſment.

After this I conceived that the great art of condenſing the vapours was confined to enlarging the beak of the capital, and carefully cooling the worm. By theſe means the vapours eſcape from the ſtill with greater facility, as they are admitted into the worm by the ſpeedy condenſation of thoſe which preceded them.

Theſe different improvements began to be introduced into Languedoc about fifteen years ago. The brothers Argand greatly contributed to make them be adopted. They firſt eſtabliſhed ſtills according to theſe principles, and found them attended with ſo much ſaving, in regard to time and fuel, that from the reſults of comparative experiments it has been eſtimated at four-fifths.

I have myſelf ſuperintended ſeveral eſtabliſhments of this kind, where the ſame principles were adopted. In my opinion, it is hardly poſſible to carry improvement any further; and it is to be wiſhed that this method of diſtillation ſhould become general.

But it is not ſo much to the form of the apparatus as to the conſtruction of the fire-place, and the proper management of the fire, that theſe extraordinary effects are to be aſcribed. The poſterior edge of the grate ought to correſpond to the middle of the bottom of the boiler, that the flame may fall upon and heat the whole bottom in an uniform manner. The diſtance between the boiler and the grate ought to be from 16 to 18 inches when coals are employed; and the chimney ought to be ſpiral.

Beſides ſaving in regard to time, fuel, labour, &c. this form of apparatus has an influence on the quality of the ſpirit. It is far ſweeter than any other, and has no empyreumatic taſte; which is a fault almoſt inſeparable from the common ſpirits uſed in commerce: this quality, which renders ſpirits of this kind ſo ſuperior to others, had almoſt become cauſe of their being rejected, becauſe the inhabitants of the north, whoſe principal beverage this liquor is, found it too ſweet. It was therefore neceſſary to mix it with *burnt* ſpirit, in order to give it a good name. This fiery taſte may be eaſily communicated to it by maintaining the diſtillation beyond the uſual time. The liquor which paſſes over towards the end of the operation has evidently a burnt taſte.

In the arts, it is neceſſary to humour the taſte and even the caprice of the conſumer; and what among us is rejected in conſequence of its bad taſte, may appear exquiſite and delicious to an inhabitant of the north. Burning liquors, which in the north might be accounted weak, are too powerful for the ſenſible nerves of the inhabitants of the ſouth. Monteſquieu has obſerved with great ingenuity, that to give a Muſcovite ſenſibility you muſt flay him.

By the comparative experiments I have been able to make I am convinced that a little more ſpirit is obtained by this method than by the old one; the cauſe of which is, that the ſpirit iſſues cool from the apparatus, and ſuſtains no loſs by evaporation. The diſtilleries therefore in which this improved apparatus is employed have no ſenſible ſmell of the ſpirit.

When wines are diſtilled, the operation is carried on till the moment when the liquor which paſſes is no longer inflammable.

Wines furniſh more or leſs ſpirit, according to their degree of ſtrength. Very generous wine furniſhes a third of its weight. The mean term of the product of our wines in the ſouth is a fourth of the whole: there are ſome which furniſh a third.

Old wines give better ſpirit than the new; but they furniſh a leſs quantity, eſpecially when the decompoſition of the ſaccharine body has been terminated before the diſtillation.

What remains in the boiler after the ſpirit is extracted is called *vinaſſe:* it is a confuſed mixture of tartar, colouring matter, dregs, &c. This reſiduum is thrown away as uſeleſs; yet by drying it in the air, or in a ſtove, very pure alkali might be extracted from it by combuſtion.

In ſome diſtilleries the *vinaſſe* is acidified, in order to be diſtilled, and to extract from it the little vinegar that has been formed in it.

The ſpirit is ſtronger according as it is mixed with a greater or leſs quantity of water; and as it is of importance in commerce to be able to aſcertain eaſily the degrees of ſtrength, many reſearches have been made to diſcover the means of determining it.

The diſtiller judges of the ſtrength of the ſpirit by the number, ſize, and permanency of the bubbles which are formed when the liquor is ſhaken: for this purpoſe he pours it from one veſſel into another, lets it fall from a certain height, or, what is more common, puts it into a long flaſk, which is filled two-thirds, and then ſhakes it violently, keeping the mouth ſhut with his finger. This laſt apparatus is called in French *la ſonde.*

The proof by combuſtion, in whatever manner performed, is very defective. The regulation of 1729 preſcribes putting gunpowder into a ſpoon, covering it with liquor, and ſetting fire to it. The ſpirit, if it inflame the powder, is conſidered to be of the firſt quality: when the contrary happens, it is bad. But liquor of the ſame quality inflames, or does not inflame,

flame, according to the proportion employed. A ſmall quantity always inflames; a large one never, becauſe the water which the liquor leaves is then ſufficient to moiſten the gunpowder, and to ſecure it from inflammation.

Recourſe has been had to ſalt of tartar (carbonate of potaſh) for trying the ſtrength of ſpirits. This alkali diſſolves in water, but not in alcohol, ſo that the latter floats on the ſolution which is formed.

Theſe methods, more or leſs defective, have rendered it neceſſary to try other means capable of determining the ſtrength of ſpirit, by aſcertaining the ſpecific gravity.

A drop of oil poured upon alcohol fixes itſelf on the ſurface, or is precipitated to the bottom, according to the degree of the ſtrength of the liquor. This method was propoſed, and adopted by the Spaniſh government in 1770: it formed the object of a regulation; but it is liable to error, ſince the effect depends on the height of the fall, the gravity of the oil, the ſize of the drop, the temperature of the atmoſphere, the dimenſions of the veſſels, &c.

In 1772 this important object was reſumed by two able philoſophers, Borie and Poujet, of Cette, who made known to the diſtillers of Languedoc a new areometer, which they adopted. They applied to it a thermometer, the different degrees of which indicate at every inſtant the corrections that ought to be made in the graduation of the areometer, in conſequence of the very variable temperature of the atmoſphere.

By the help of this areometer, not only may the degree of ſtrength be aſcertained, but the ſpirit may be carried to any point required; for this purpoſe different weights are provided. The heavieſt is marked *Dutch proof*, the lighteſt *three-ſevenths*: if the weight marked Dutch proof be therefore ſcrewed to the lower extremity of the ſhank, and if the inſtrument be immerſed in liquor of *three-ſevenths*, it will ſink a great deal too much; but it may be brought back to Dutch proof by adding four-ſevenths of water.

On the other hand, if the weight *three-ſevenths* be ſcrewed to the ſhank, and if the areometer be then immerſed in liquor Dutch proof, it will riſe above the laſt term, and may be eaſily brought back to that degree by adding ſtronger alcohol.

When ſpirit is diſtilled for the purpoſe of extracting alcohol from it, a *balneum mariæ* is commonly employed: the heat is then more gentle, and more uniform, and the product of the diſtillation is of a better quality: this product is the common ſpirit of wine uſed in commerce.

3d, *Tartar.* Tartar exiſts in verjuice: it is found alſo in muſt: it concurs to facilitate the formation of alcohol, as we have already obſerved from the experiments of Bullion. It depoſits itſelf on the ſides of the caſks in conſequence of reſt, and forms there a cruſt of greater or leſs thickneſs, rough, with cryſtals very badly determined. Some time before the vintage, when the veſſels are getting ready for receiving it, the caſks are opened, and the tartar is taken out, to be employed in commerce and for various other purpoſes.

Tartar is not furniſhed by all wines in the ſame proportion. Red wines give more than the white. The higheſt coloured and thickeſt generally give the moſt.

The colour of the tartar alſo varies very much, and it is called *red* or *white*, according as it is produced from either of theſe wines.

This ſalt is very little ſoluble in cold water: it is much more ſo in boiling water. It ſcarcely diſſolves in the mouth, and reſiſts the preſſure of the teeth.

It is freed from its colouring principle by a ſimple proceſs, and is then known under the name of *cream of tartar.* For this purpoſe it is diſſolved in boiling water, and, when the water is ſaturated, the ſolution is put into earthen veſſels to cool: as it cools, it precipitates a ſtratum of cryſtals which are already almoſt free from colour. Theſe cryſtals are again diſſolved in boiling water: four or five per cent. of an argillaceous and ſandy kind of earth, found at Murviel, near Montpellier, is diffuſed through the ſolution, and it is then evaporated to a pellicle. On cooling, it precipitates white cryſtals, which being expoſed in the open air on cloths for ſome days, acquire that whiteneſs which belongs to cream of tartar. The mother-waters are reſerved for being employed in new ſolutions. Such is the method practiſed at Montpellier and in its environs, where the principal part of the manufactories of cream of tartar are eſtabliſhed.

The tartar is employed alſo as a flux: it is attended with the double advantage of furniſhing the carbon neceſſary for the deoxygenation of the metals, and alſo alkali, which is one of the beſt fluxes known.

Tartar is purified alſo by calcination. Its acid is decompoſed and deſtroyed by this proceſs, and there remains then nothing but the alkali and the charcoal: the alkali is diſſolved in water, the liquor is filtered, and, being then concentrated, that ſalt known in the ſhops under the name of *ſalt of tartar*, or *carbonate of potaſh*, is obtained.

The alkali furniſhed by tartar amounts only to one-fourth of its weight.

4th, *Extractive matter.* The extractive principle abounds in muſt. It appears to be diſſolved in it by the help of the ſugar; but when the fermentation changes the nature of the ſaccharine principle, the extractive matter ſenſibly decreaſes. A portion brought back then almoſt to the ſtate of fibrine matter is precipitated: the depoſit is the more ſenſible as the fermentation ſlackens, and as the alcohol is more abundant. This is what conſtitutes in particular the lees. Theſe lees are always mixed with a pretty conſiderable quantity of tartar, which it envelops.

There always exiſts in wine a portion of extractive matter which is in perfect ſolution, and from which it may be freed by evaporation. It is more abundant in new wines than in old. The older they grow, they ſeem to be the more completely freed from it.

Theſe lees, after being dried in the ſun or in a ſtove, are well preſſed, and then burnt, to extract that ſort of alkali called in commerce *wood aſhes.* The combuſtion is effected in a furnace, the ſides of which are raiſed in proportion as it takes place: the reſiduum is a porous maſs, of a greeniſh gray colour, which forms about the thirtieth part of the lees burnt.

Wines are freed from theſe lees by drawing them off, in order to preſerve them from acid degeneration.

5th, *Aroma.* All natural wines have an odour more or leſs agreeable. There are ſome, even, which are indebted for a great part of their reputation to the agreeable odour they exhale. This is the caſe with Burgundy wine. This perfume is loſt by too tumultuous a fermentation: it becomes ſtronger by age. It rarely exiſts in very generous wines; either becauſe it is concealed by the ſtrong odour of alcohol, or becauſe the ſtrong fermentation which has been neceſſary to develop the ſpirit has extinguiſhed or diſſipated it. This aroma does not appear ſuſceptible of being extracted, to be communicated at pleaſure to other ſubſtances. Heat even ſeems to deſtroy it; for, except the firſt liquid which paſſes over by diſtillation, and which retains a little of the odour peculiar to wine, the ſpirit which comes after has no longer the characters that eſſentially belong to it.

6th, *The colouring principle.* The colouring principle of wine exiſts in the pellicle of the grapes. When the muſt is fermented without the ſkins, the wine is white. This colouring principle does not diſſolve in the vintage but when the alcohol is developed in it; and it is only then that the wine becomes coloured. The colour is ſtronger according as the fermentation has been more tumultuous, or as it has been left longer in the vat. Mere preſſure of the grapes by treading them, if done with care, may mix with muſt a quantity of colouring matter ſufficient to make the maſs aſſume a very intenſe colour. When it is required to obtain wine free from colour, the grapes are collected while the dew is upon them, and they are trod as little as poſſible.

The colouring principle is in part precipitated in the caſks along with the tartar and the lees; and when the wine is old, it is not uncommon to ſee it entirely free from colour. The colour then depoſits itſelf in pellicles on the ſides of the veſſels, or at the bottom: it is ſeen floating in the liquid like films, which render it turbid.

If bottles filled with wine be expoſed to the ſun, a few days will be ſufficient to precipitate the colouring matter in large pellicles; and the wine loſes neither its perfume nor its qualities. I have often made this experiment on the old highly coloured wines of the ſouth.

To precipitate the principle of the colour, nothing is neceſſary but to pour lime water into the wine. In this caſe, the lime combines with the malic acid and forms a ſalt, which appears in the liquor under the form of light flakes. Theſe flakes gradually depoſit themſelves, and carry with them all the colouring principle. The depoſit is black or white, according to the colour of the wine employed for the operation. It often happens that wine is ſuſceptible of forming a precipitate, though it has been completely freed from its colour by a firſt depoſit; which proves that the principle of the colour has ſtrong affinity to malate of lime. The coloured precipitate is inſoluble both in cold and in warm water. This liquid even produces no change in the colour. Alcohol has ſcarcely any effect upon it, only it acquires from it a ſlight tint of brown.

The nitric acid diſſolves the colouring principle of this precipitate.

When wine has been reduced to the ſtate of extract, the alcohol poured over it becomes ſtrongly coloured, as well as the water, though in a leſs degree. But, beſides the colouring principle which is then diſſolved, there is alſo a ſaccharine extractive principle which facilitates the ſolution.

The colouring principle, then, does not appear to be of the nature of reſins; it preſents all the characters belonging to a very numerous claſs of vegetable products, which approach very near to feculæ, without having all their properties. The greater part of colouring principles are of this kind: they are ſoluble by the aſſiſtance of extractive matter, and when freed from this medium they become fixed in a laſting manner.

LXI. *Some Account of* C. Fourcroy.

THE following account of this chemiſt is extracted from *Schmeiſſers Beyträge zur näheren kentniſſe des gegenwärtigen Zuſtandes der Wiſſenchaften in Frankreich.*

"Fourcroy, who, as I believe, is the firſt public teacher of the modern chemiſtry in Europe, is ſo well known to the literary world by the ſervice he has rendered to that ſcience, and by the many excellent works he has written on the ſubject, that I can no further contribute to extending his fame than by aſſuring the public that this meritorious man, notwithſtanding the employment he has as a member of the Council of Elders, continues to cultivate his favourite purſuit with unabated zeal.

"He indeed continues to lecture on chemiſtry with great zeal; and his deep knowledge of the ſcience has been ſo much enriched by the rapid progreſs it has lately made, that the great ſtore of facts he poſſeſſes, and which he explains to a crowded audience with great elegance and beauty of language, often obliges him to exceed the time he uſually allots for that purpoſe. I, however, never found that he either fell ſhort of matter, or that the patience of his hearers was exhauſted, though he reads every day, and on ſome days three lectures, on different branches of chemiſtry.

"When we reflect on the various labours which his public duties require, it may readily be conceived how few unoccupied hours he has left for his daily lectures and chemical experiments. The principal places which he occupies are the following: he is a member of the Council of Elders and of the National Inſtitute; of the Society for promoting the Arts and Sciences; of the Council of Health; profeſſor of chemiſtry in the Republican Lyceum, at the National Muſeum of Natural Hiſtory, and the General School of Arts. His indefatigable aſſiſtant Vauquelin, whoſe head and hands are entirely employed with experiments, ſupplies him with abundance of chemical diſcoveries, which he either confirms or has made himſelf.

"Sometimes I paid an early viſit to Fourcroy at his own houſe; for this was the only time I could have an opportunity of converſing with him on chemical ſubjects. I always met with the politeſt reception, and always found him ready to enter on a diſcuſſion of topics relating to our favourite ſtudy.

"I ſaw C. Fourcroy's own chemical laboratory, and the hall in which he gives lectures on natural hiſtory. It is not very large, but well fitted for his purpoſe, and furniſhed with apparatus and every other thing that can ſerve for illuſtrating his lectures. I found him much employed at his own houſe on that part of chemiſtry which he has engaged to write for the new Encyclopédie, and which, as I ſaw by his manuſcript, is nearly finiſhed, and will no doubt meet with the approbation of the public. He informed me, that as ſoon as this work was completed he intended to begin another, viz. A Hiſtory of the Revolution effected in France in regard to Chemiſtry, and the Foundation of the Theory reſpecting the different Kinds of Gaſes. Fourcroy was one of thoſe who chiefly contributed to the eſtabliſhment of thoſe learned inſtitutions and ſchools which at preſent exiſt at Paris, and he ſtill labours for their improvement. His principal works are: An Eſſay on the Diſeaſes of Artiſts, by Ramazzini, 1777, 12mo. The Principles of Chemiſtry, 2 vols. 8vo. of which there have been ſeveral editions: the laſt conſiſts of eight volumes. The Principles of Chemiſtry, for the Uſe of the Veterinary School, 2 parts, 16mo. A Treatiſe on the acidulous Springs of Engheim. A Collection of Memoirs in Chemiſtry, 8vo. The Art of knowing and employing Medicines, 2 vols. 12mo. On the Improvement of Medicine by the Phyſical Sciences, 1792 and 1793, 4 vols. 8vo. The Philoſophy of Chemiſtry. And many ſingle papers in the Memoirs of the Academy of Sciences and the Annals of the Society of Medicine; ſome public orations on the ſciences, and a few diſſertations on ſaltpetre.

“ Fourcroy is a man about 40 years of age*, of an agreeable aſpect, and great activity. At preſent he is ſo much engaged with public buſineſs, that he has little time left for domeſtic enjoyment; he has therefore ſent his wife to a country retreat, and placed his eldeſt ſon under the care of his friend Vauquelin, at whoſe houſe he enjoys the inſtruction of Stany.

“ Fourcroy in the courſe of his converſation ſeemed to have a great attachment to England, on account of the great progreſs which the ſciences have made in that country, and he often expreſſed a deſire of paying a viſit to it on the reſtoration of peace.”

LXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

On the 24th of December was read a paper on friction, by Mr. Southern. He made a number of experiments on the motions of large grindſtones revolving with great rapidity, and aſcertained the power of friction on their axes from the number of revolutions which they performed when ſet in motion with various velocities. He conſiders the reſults as fully confirming Mr. Vince's principles, that friction is a uniformly retarding force; although the reſiſtance of the air and other accidental circumſtances introduced great irregularities into the experiments. He found this force equal to about 1-40th of the weight: the ſteel ſpindles running on braſs, with the interpoſition of an unctuous ſubſtance.

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* This was written in 1797.

The Society adjourned to Thurſday the 14th of January, when an algebraical paper was read, of which we can give no idea in a ſhort notice.

A paper, by Smith Gibbs, Eſq. on galvaniſm, has alſo been read before the Society; but we have occupied ſo large a portion of our preſent Number with that ſubject, that we cannot at preſent enter upon it.

ACADEMY OF THE USEFUL SCIENCES AT ERFURT.

The Academy of the Uſeful Sciences at Erfurt, in conſequence of a prize of thirty ducats, offered by an anonymous friend of chemiſtry, have propoſed the following queſtion: What uſeful application can be made in chemiſtry and the arts of the temperatures below zero of Reaumur; and how far is it poſſible to lower the temperature by artificial means? The Society wiſhes that thoſe who undertake to anſwer this queſtion will repeat the experiments of Lowitz, Fourcroy, &c. and examine whether a much greater degree of cold than any yet found may be produced by changing the proportions of the mixtures, employing them in greater quantity, by new mixtures, or by placing more refrigeratory veſſels within each other, and, where it can be done, by making experiments in a rarefied atmoſphere: alſo to examine the influence of low temperatures on the different gaſes ſhut up with ſpirit of wine; and to obſerve their mixture, where poſſible, in a compreſſed ſtate; and their influence alſo on liquids, on ſolid bodies, and on the galvanic phænomena: and, in the laſt place, to ſhow what employment may be made of the low temperatures in the arts and manufactures.

The papers on this ſubject muſt be tranſmitted to profeſſor Bellerman, the ſecretary of the academy, before the end of April 1802; three months after which the prize will be adjudged.

FRENCH NATIONAL INSTITUTE.

The National Inſtitute of France having been formed during the war, the nomination of the foreign aſſociates, inſtituted by the ſecond article of the law of 3d *Brumaire* (Oct. 25.) year 4, was ſuſpended. As peace has removed this ſuſpenſion, in the General Sitting, which brings together, on the 5th of each month, the three claſſes of the

Inſtitute, each of them preſented the names of three candidates; from which the Inſtitute, on the 5th *Nivoſe* (26th December) were to chooſe a firſt aſſociate for each claſs.

The three candidates preſented by the claſs of the Mathematical and Phyſical Sciences are, Sir Joſeph Banks, Dr. Maſkelyne, and Dr. Herſchel.

The three candidates of the Moral and Political Sciences—Mr. Jefferſon, Preſident of the United States, Major Rennel, Count Rumford.

The three candidates named by the Claſs of Literature and the Fine Arts are, M. Haydn, Mr. Sheridan, and Klopſtock.

Each claſs, in order to preſent its three candidates for election to the three claſſes united, was itſelf obliged to elect from a greater number of candidates preſented to it by a commiſſion compoſed of a member from each ſection. The liſt of the candidates propoſed to each claſs by the commiſſion was as follows:—

For the firſt claſs—Dr. Maſkelyne, Mr. Watt, Dr. Herſchel, Dr. Prieſtley, Mr.Cavendiſh, Profeſſor Volta, Sir Joſeph Banks, Profeſſor Pallas, Maſcagni, Arthur Young, Eſq.

For the ſecond claſs—Mr. Jefferſon, Count Rumford, Norman, Kant, Herder, Sir John Dalrymple, Profeſſor Dugald Stuart, Sir John Sinclair, Ebling, Arthur Young, Eſq. Rhabec, Marini, Gaetano, Librarian of the Vatican, Champomann, Lardizobal, David Ramſay, Niebuhr, Horne Tooke, Mr. Fox.

For the third claſs—Arnold, Cannova the Italian ſculptor, Calderari, Ceſarotti, Italian poet; Haydn, celebrated muſician; Heyne, Klopſtock, Merian, of the Academy of Berlin; Sergelle, a Swede; Sarti, celebrated Italian muſician; Mr. Sheridan, Horne Tooke.

In the General Sitting, on the 26th of December, the Inſtitute proceeded to the election of the three aſſociated members; when Mr. Jefferſon, Preſident of the United States of America, was choſen for the Claſs of Moral and Political Science. Major Rennel had 157 votes, and Count Rumford 169.

For the claſs of Phyſical and Mathematical Science, Sir Joſeph Banks, Preſident of the Royal Society of London, was nominated; and for the Claſs of Literature and the Fine Arts, M. Haydn.

Account of the labours of the Claſs of the Mathematical and Phyſical Sciences during the firſt quarter of the year 10. *Read by* C. Delambre, *ſecretary.*

Aſtronomy.—The ſtar diſcovered about a year ago by Piazzi at Palermo has hitherto eſcaped the reſearches of all aſtronomers. Similar, in regard to the ſplendour of its light, to a ſtar of the ſeventh or eighth magnitude, it exhibited none of thoſe appearances that ſerve to diſtinguiſh comets. In colour it reſembled Jupiter; and, from obſervations made in the meridian by M. Piazzi and his aſſiſtant M. Cacciatore, it would ſeem that this ſtar is a planet, the orbit of which gives reaſon to ſuppoſe that its revolution is performed in four years and a half, or five years. Towards the end of *Pluvioſe*, as this ſtar paſſed the meridian too early, it ceaſed to be viſible; and M. Piazzi, aſſiſted by Cacciatore and Cariotti, both endowed with excellent ſight, and well acquainted with the heavens, endeavoured to find it, but in vain, both with a night-glaſs and an achromatic teleſcope of a large aperture. It needs therefore excite no ſurpriſe that, nine months after, all the aſtronomers of Europe ſhould have failed in their reſearches, ſince to the difficulty of the thing itſelf there was added an uncertainty of ſome degrees reſpecting the exact place where it was neceſſary to ſearch for this ſtar, as the elements of its orbit, calculated on too ſmall an arc, cannot, after ſo long an interval, give, in a ſufficiently correct manner, the geocentric places. Beſides, the heavens have been almoſt always cloudy; and to be able to hope for ſucceſs, it would be neceſſary, conſidering the ſmall ſize of this ſtar, to make a correct enumeration of all the ſtars from the ſeventh to the ninth magnitude in the neighbourhood of which it may appear, and to repeat the examination of them every day until the ſmall planet ſhould diſcover itſelf by its motion. It will ſoon be under the ſame circumſtances in which it was at the time of its diſcovery; and if the heavens ſhould become more ſerene, we may ſtill retain ſome hope.

Winter Solſtice of the year 10. (Dec. 1801.)—The weather, ſo unfavourable to the reſearches made to obſerve Piazzi's planet, was no leſs ſo to the determination of the ſolſtitial altitude of the ſun, which was conſtantly concealed from the 19th to the 26th of December; that is to ſay, during thoſe days on which the moſt concluſive obſervations could have been made. However, by collecting thoſe made on the preceding and following days, C. Delambre was able to form nine ſeries of zenith diſtances, obſerved by Borda's circle, which gave him for mean reſult an apparent obliquity of 23° 28′ 3″. This quantity holds nearly a mean place between thoſe found ſeveral years ago by the winter ſolſtices on the one hand, and the ſummer ſolſtices on the other. On this occaſion the obſervation made at the winter ſolſtice approached very near that made at the ſummer ſolſtice. It is well known, that for a long time paſt all aſtronomers who have employed themſelves in determining the obliquity of the ecliptic have always found ſeveral ſeconds leſs in winter than in ſummer. On this occaſion, however, the obſervation made at the winter ſolſtice approached very near to that made at the ſummer ſolſtice. We ſhall not attempt to explain the cauſe. We ſhall content ourſelves with mentioning the fact; but we muſt add, that, according as the thermometer by which the refraction is corrected is poſited within or without the obſervatory, or near the teleſcope, there will be a ſecond leſs or more in the above determination for which the mean of theſe thermometers was taken.

The phyſical part was read by Lacepede, ſecretary.

A commiſſion, conſiſting of Laplace, Coulomb, Hallé, Monge, &c. having been appointed to examine the phænomena of galvaniſm, and to repeat the experiments of profeſſor Volta, they communicated the reſult in a report, in which they explained the theory, and its identity with electricity. (For this report, ſee page 301.)

LXIII. *Intelligence and Miſcellaneous Articles.* *January* 1802.

MINING.

THE proſpect of peace has made conſiderable alteration in the views of the miners of Cornwall and Devon. The expected reduction in the prices of the articles of which ſuch vaſt quantities are conſumed in their works, as timber, gunpowder, iron, &c. and the probable increaſe in the value of tin at leaſt, and perhaps of copper, have given peculiar energy to the carrying on the preſent mines, and to exertions for new diſcoveries. Perhaps the ſpirit of adventure in theſe undertakings was never much higher.

Among the new diſcoveries is that of a courſe of copper ore, in the eaſtern part of Wheal Fortune mine, in the pariſh of Gwennap, Cornwall. And the ſame has been found to extend into the infant concern of Wheal Friendſhip, a mine adjoining on the ſame lode. Penandrea, under the town of Redruth, is throwing up a good deal both of tin and copper, and the extenſive mines of North Downs are ſaid to be rich in the upper levels, though the bottoms have been ſtopped now for ſome time.

A new copper mine is going on ſouth of Wheal Fortune, called Wheal Girl, and the old and great mine Wheal Buſy, which formerly has produced ſo much tin and copper in intimate mixture, it is reported will ſpeedily be put in courſe of working.

On the eaſtern borders of Cornwall, and in the part of Devon adjoining, mining has within a ſhort time been conſiderably extended. Wheal Crowndale, in the pariſh of

Taviſtock, lately diſcovered, is already producing a conſiderable quantity of copper ore, as alſo are ſome other mines of not long ſtanding in the neighbourhood. A new mine is going on, on Wheal Crowndale lode, to the weſt of that concern.

Among minerals found of late, may be mentioned *fluate of lime*, cryſtallized in cubes, produced in fine pieces from Gunnis-Lake mine (copper lode running in granite) on the Cornwall banks of the Tamar, and ſome ſpecimens of ſcaly iron ore incumbent on cryſtals of quartz from Wheal Crowndale. Some quantity of the rare ſpecies of fluors cryſtallized in tetrahedral priſms has of late been collected from the waſte at the old lead and ſilver mines, not now at work, at Beerfuris in Devon.

ANTIQUES.

Mr. Holland, the architect, is in poſſeſſion of a fine collection of antique fragments purchaſed for him at Rome by Mr. Tatham previous to the revolution. It conſiſts of ſome of the beſt ſpecimens of ornamental ſculpture, and may therefore be conſidered as an acquiſition to the country, as the ſtudy of ſuch models ſerves in an eminent degree to form a correct taſte in ſtudents of architecture.

Dr. Garnett's Lectures on Zoonomia, or the laws of animal life, which commenced on Wedneſday, the 20th of January, have excited conſiderable attention, on account of the novelty of the ſubject. In the firſt lecture, Dr. Garnett gave a general deſcription of the human body, conſidered as a machine conſiſting of bones and muſcles, for the purpoſes of motion, at the inſtance of its intelligent principle. He likewiſe conſidered the various theories of ſenſation and muſcular motion, and concluded the lecture with ſome galvanic experiments, which ſeemed to ſhow that electricity has a conſiderable ſhare in producing theſe phænomena.

In the ſecond lecture, which was delivered on Saturday laſt, he proceeded to conſider the nature of reſpiration, and the cauſe of animal heat. At the requeſt of ſeveral friends in the City, he intends to deliver this courſe in Cornhill every Wedneſday evening.

A. and C. R. Aikin will begin a courſe of evening lectures on various chemical manufactures, and the outlines of general chemiſtry, on Monday the firſt of March next.

The lectures will be twenty-eight in number, and will be given every Monday and Friday, at the Alderſgate Street General Diſpenſary.

Further particulars may be learned at Mr. Aikin's, No. 4, Broad Street Buildings.

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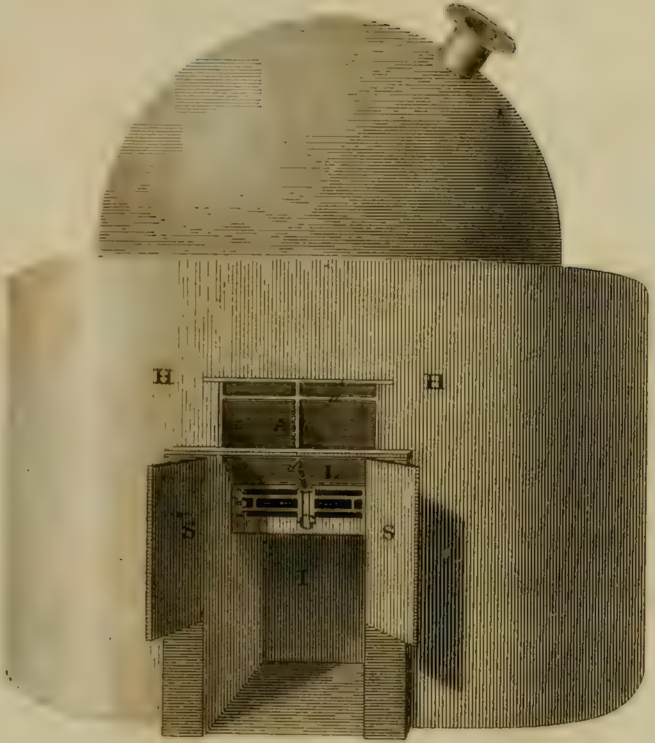

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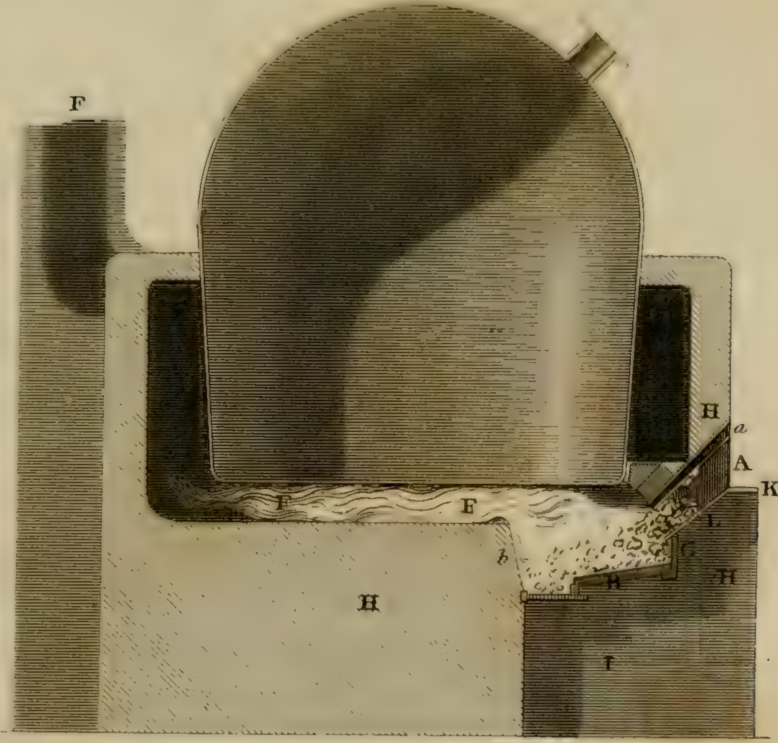

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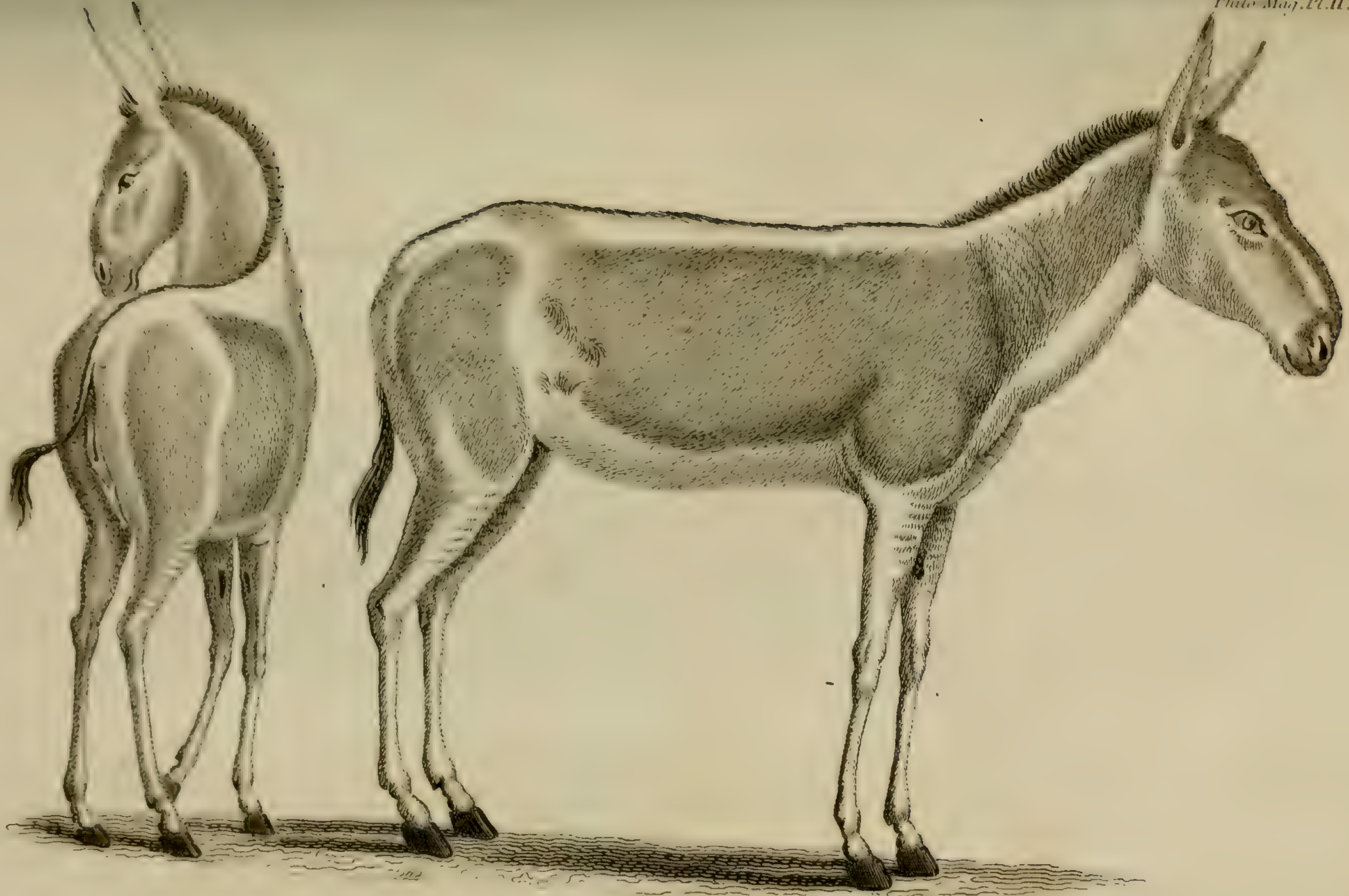

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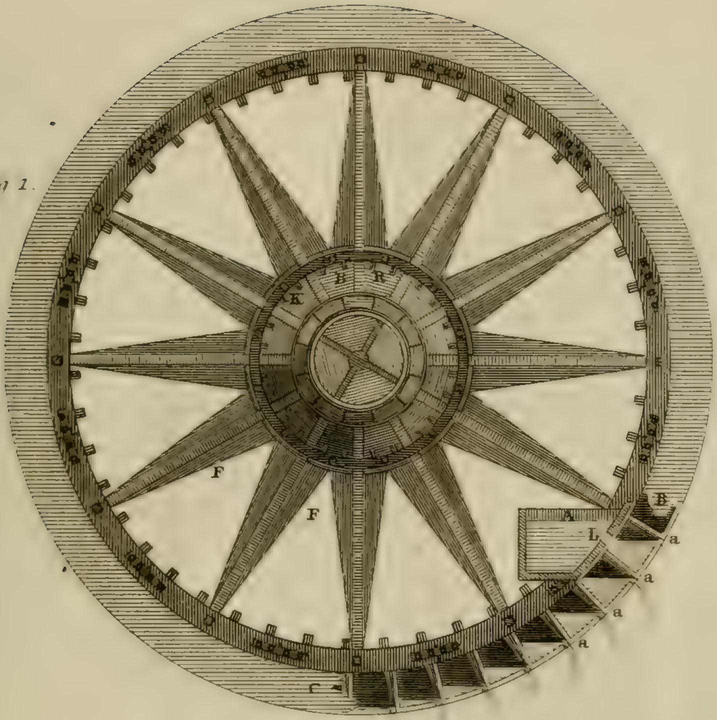

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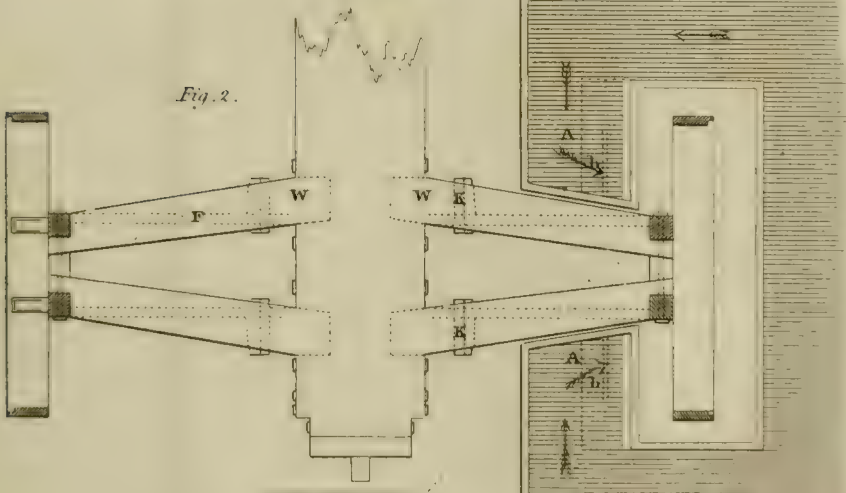

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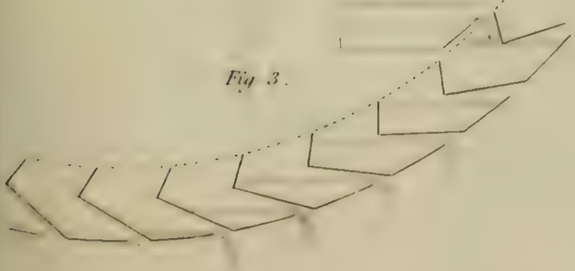

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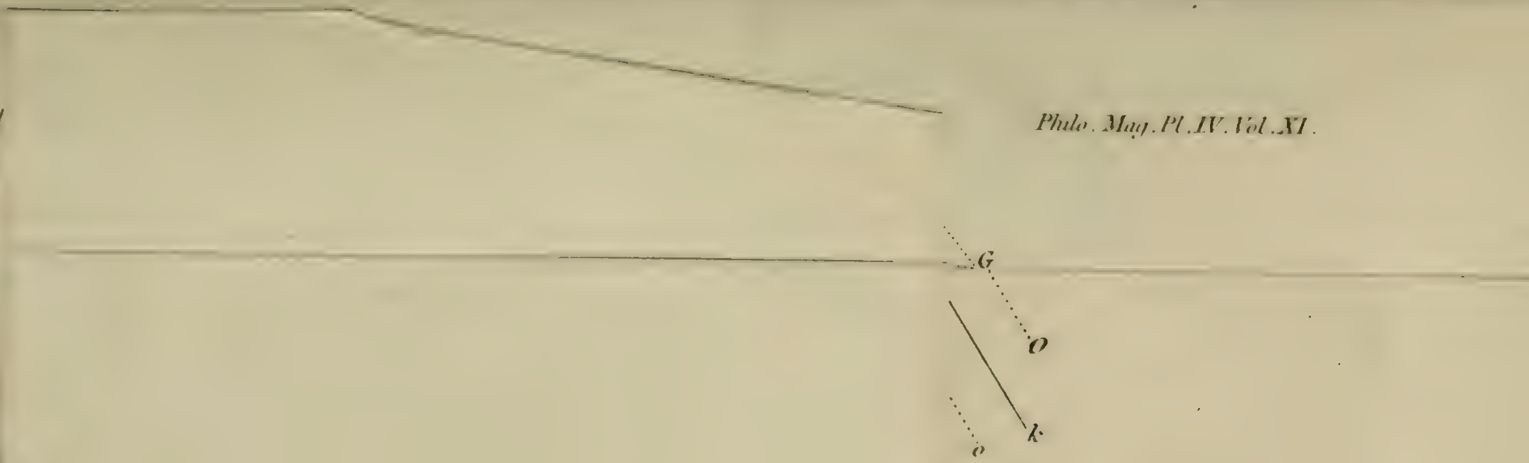

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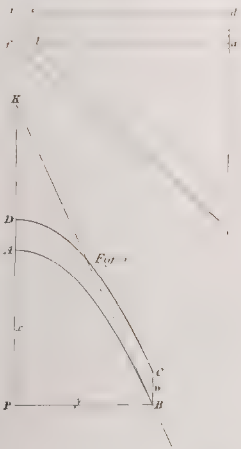
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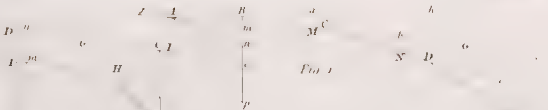
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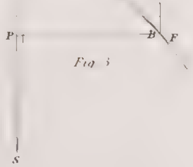
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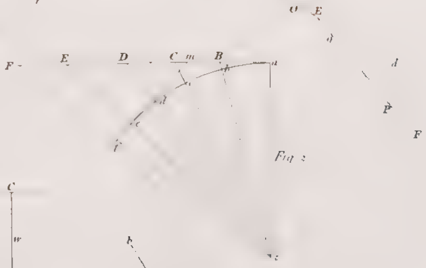
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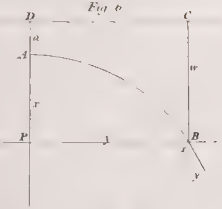
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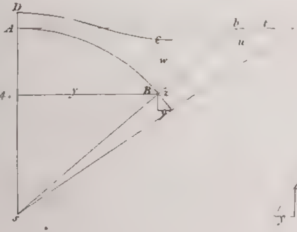
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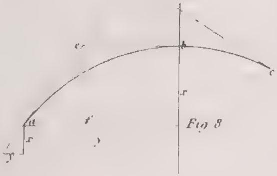
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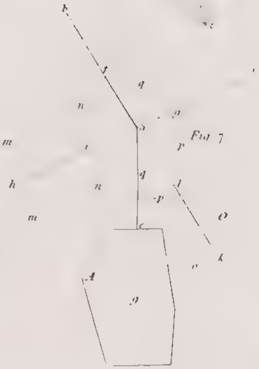
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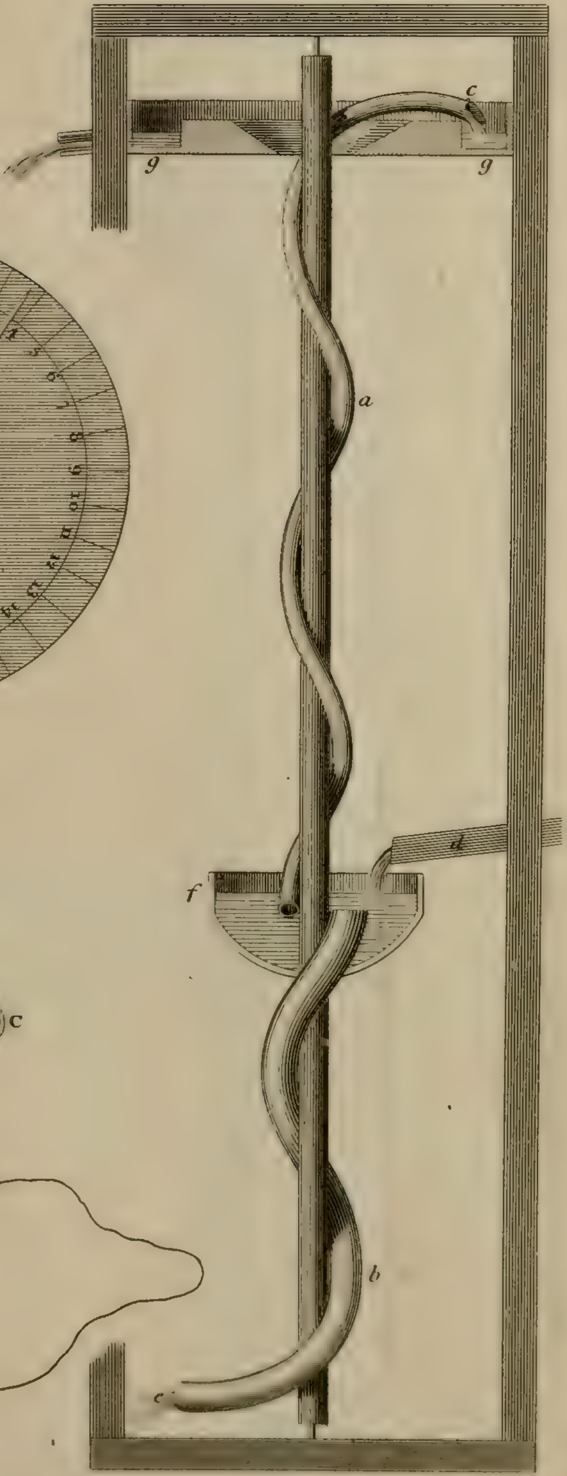

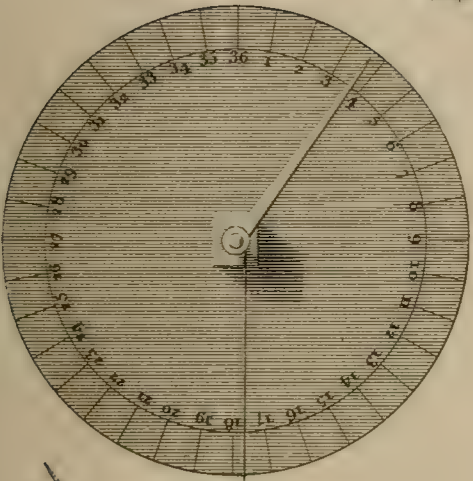

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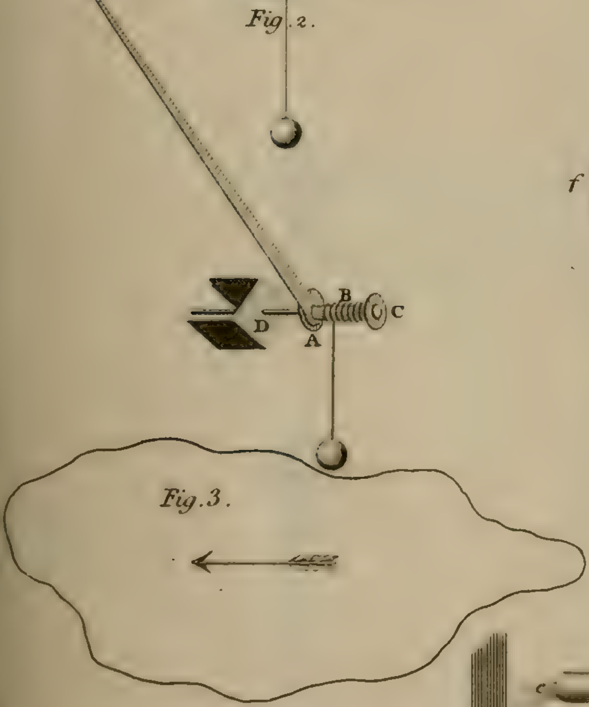

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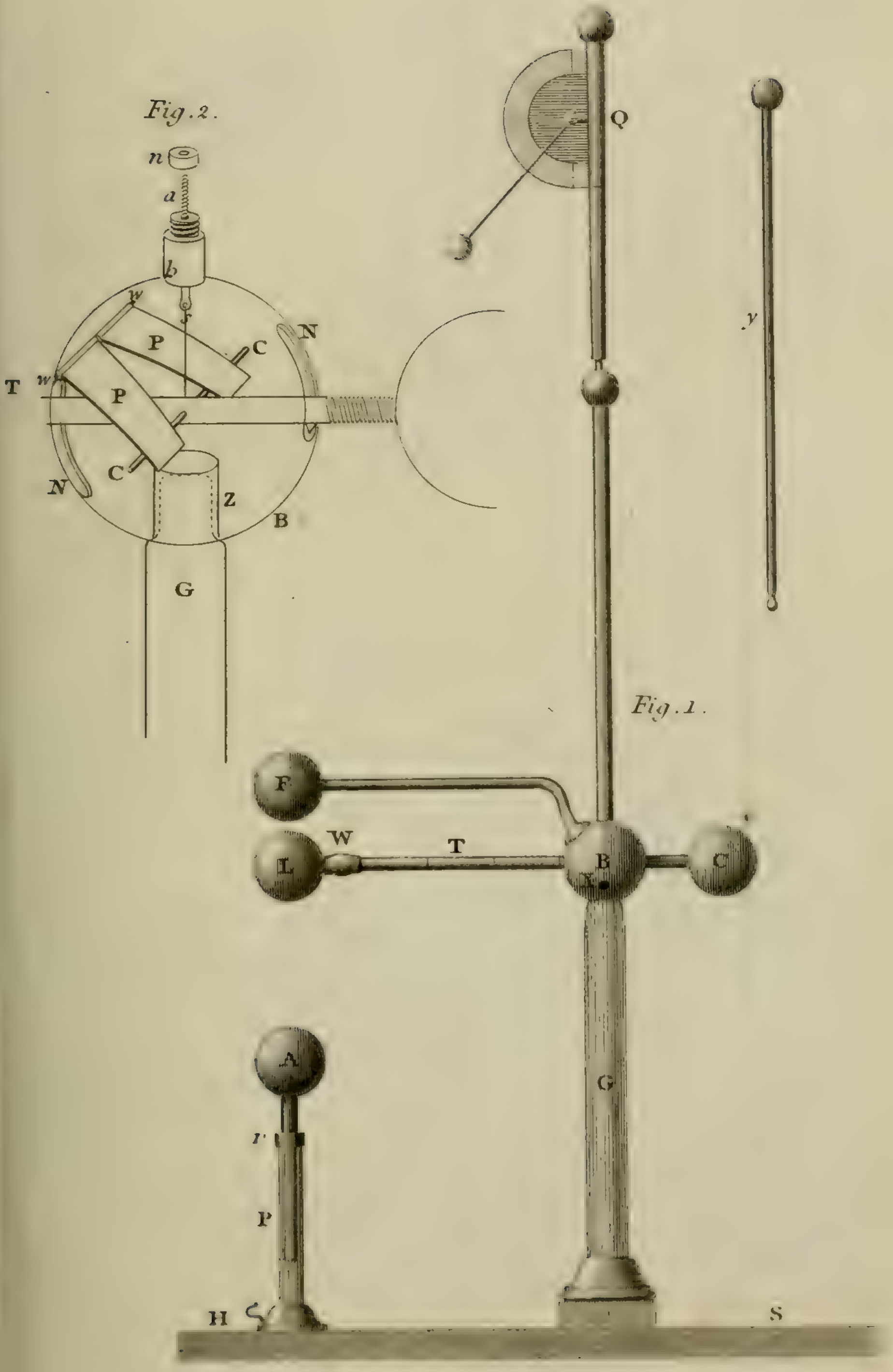

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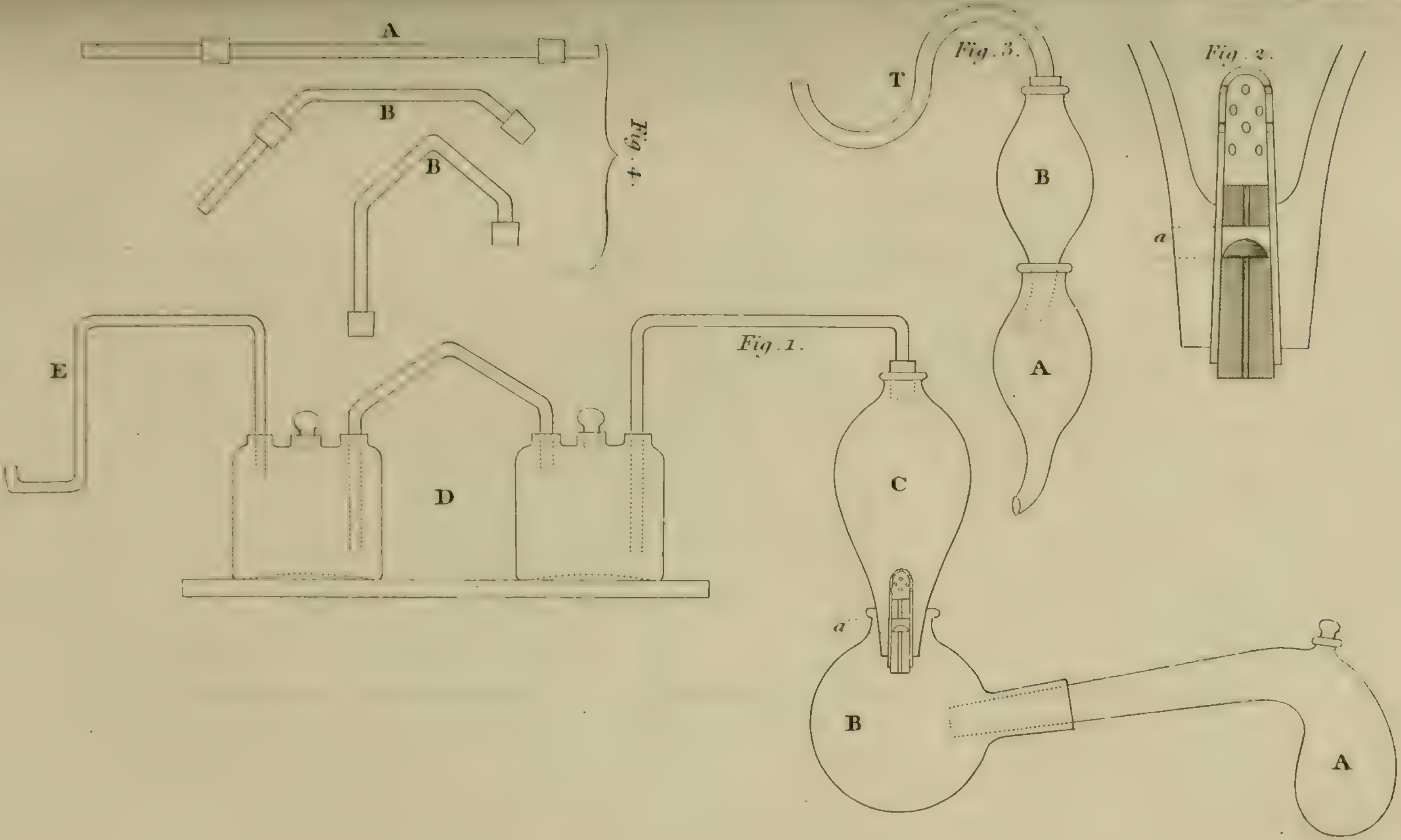

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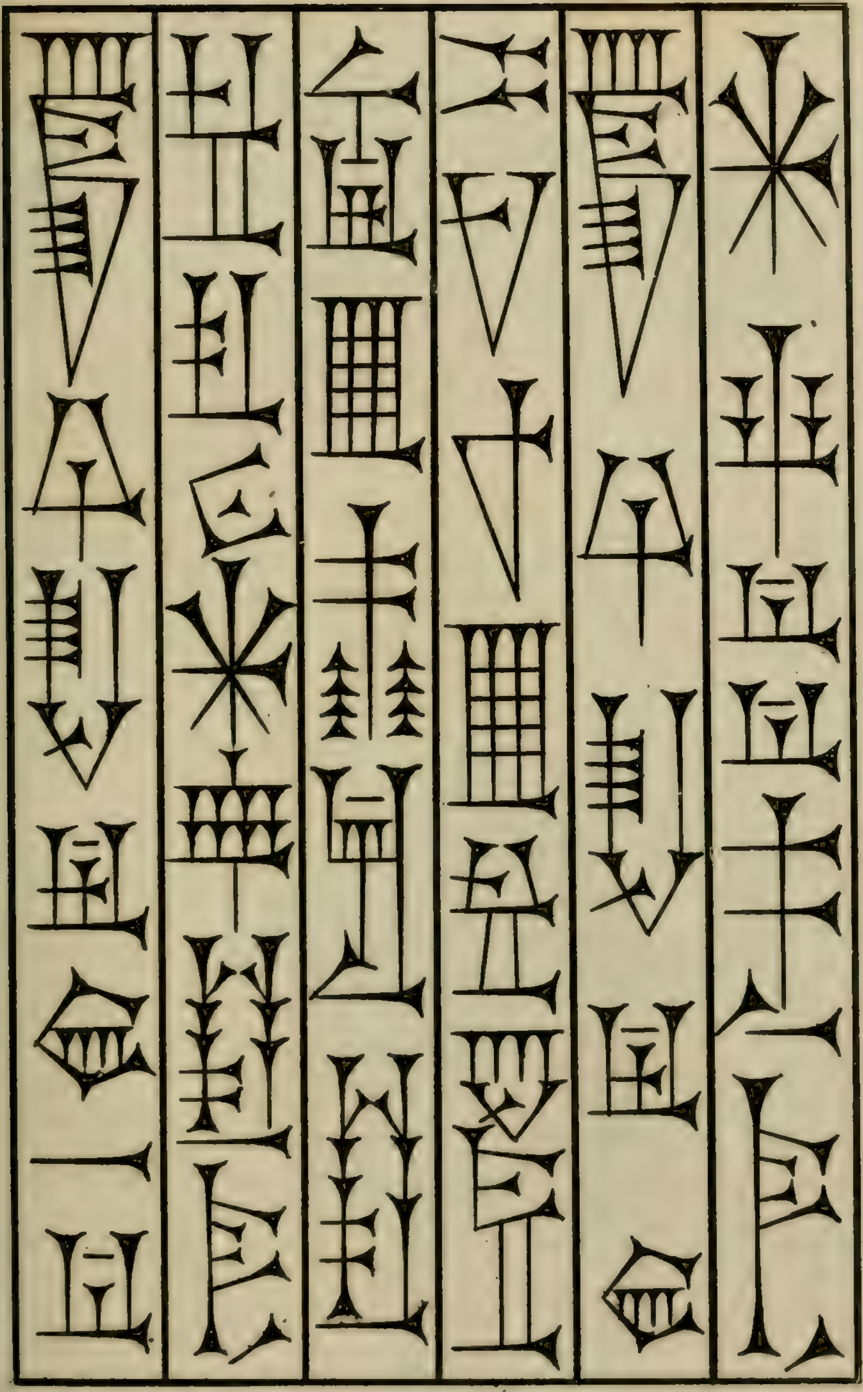

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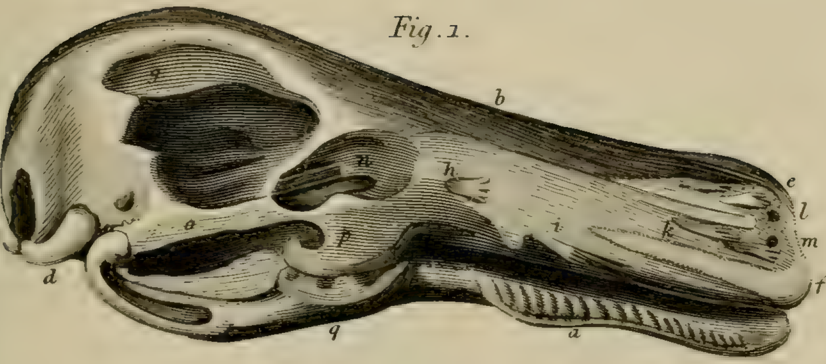

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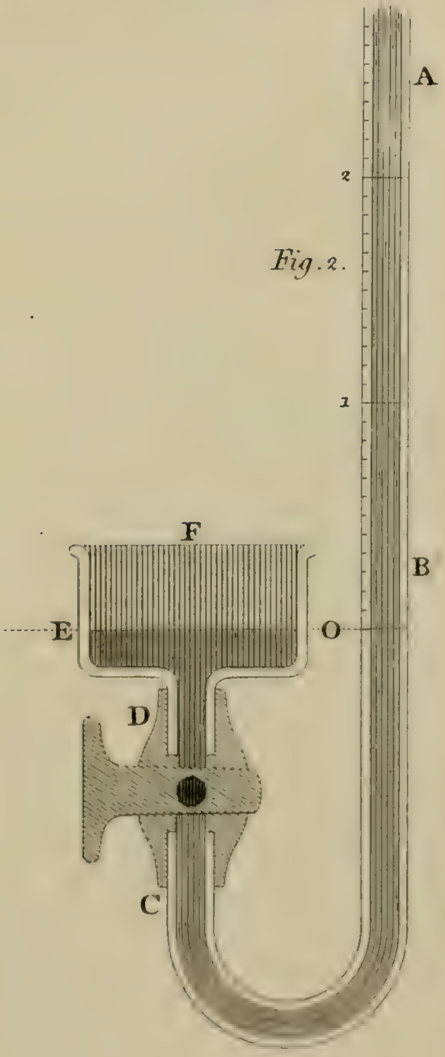

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